Entered as second class matter at the New York, N. Y. Post Office, November 26th, 1890.

VOLUME IX.
NUMBER 5 & 6.

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Annual Subscription $5.00
Single Copies - 50 cts.

MAY AND JUNE, 1892,

[DOUBLE NUMBER.]

TRANSACTIONS
OF THE
AMERICAN INSTITUTE
OF
ELECTRICAL ENGINEERS.

ANNUAL MEETING, MAY 17TH, 1892.

Reports of Council and Treasurer. Officers Elected.

Obituary—Charles Joseph Van Depoele.

GENERAL MEETING, CHICAGO, JUNE 6TH AND 7TH, 1892.

Note on Some Experiments with Alternating Currents.
BY DR. LOUIS DUNCAN.

Rational and Empirical Formulæ, Showing the Relation between the Magnetomotive Force (H) and the Resulting Magnetization (B.)
BY CHARLES E. EMERY, PH.D.

A Dynamo Indicator, or Instantaneous Curve-Writing Voltmeter.
BY PROF. GEORGE S. MOLER.

Coming Development of Electric Railways (Inaugural Address.)
BY FRANK J. SPRAGUE, President.

The Magnetic Permeability of Special Irons for Electrical Purposes.
BY MILTON E. THOMPSON.

Notes on Wiping Contact Methods for Current and Potential Measurements.
BY PROF. BENJ. F. THOMAS.

A Life and Efficiency Test of Incandescent Lamps.
BY PROF. BENJ. F. THOMAS.

Some Points for the Electrical Engineer.
BY HORATIO A. FOSTER.

PUBLISHED MONTHLY BY THE INSTITUTE, UNDER THE SUPERVISION OF THE EDITING COMMITTEE.

RALPH W. POPE, *Secretary*,
12 WEST THIRTY-FIRST STREET.
NEW YORK CITY.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

ANNUAL MEETING.

The Annual Meeting of the American Institute of Electrical Engineers, was held at 12 West 31st street, New York City, May 17th, 1892. The meeting was called to order at 4 P. M. by Vice-President Lockwood. The following annual reports of the Institute were read:

REPORT OF COUNCIL FOR THE YEAR ENDING APRIL 30TH, 1892.

Your Council takes pleasure in submitting for your information, in accordance with the Rules of the Institute, the following report, showing the growth in membership, receipts and disbursements and a brief review of the work of the year ending April 30th. It has been the practice heretofore to bring this report down to the date of the annual meeting, but it is more convenient to end the fiscal year with the calendar month, which gives more time to prepare the reports for presentation, and if there appears to be no objection to this practice, the yearly accounts will be closed hereafter on April 30th.

Ten regular meetings and one special meeting of the Council have been held during the year, at which the average attendance has been 11. The largest number present having been 13 and the smallest 7.

The general meeting of the Institute was held in New York City, May 20th and 21st, and occupied two days. Eight other meetings have been held. At these various meetings, 24 papers and reports were read and discussed, and have since been printed and distributed to the members.

The Institute was invited to unite with other engineering societies in the establishment of Engineering Headquarters at Chicago during the Columbian Exposition. As the plans of the Exposition developed, however, it appeared that the proposed Electrical Congress would require the undivided support of all electrical organizations, and for this reason your Council deemed it wise to decline entering into the proposed arrangement. It was subsequently decided that the Institute should support the action of the World's Congress Auxiliary in making preparations for the World's Electrical Congress in order to do its share in making the Congress a success.

In accordance with the instructions of a meeting of the Institute, the following delegates were appointed by Council to represent the Institute at the Frankfort Electrical Congress in 1891: Messrs. Carl Hering, Nikola Tesla, Prof. Richard O. Heinrich and Dr. Edward L. Nichols With the exception of Mr. Tesla, all the delegates were in attendance, and their report was duly submitted to Council and printed in the Transactions.

At the meeting of February 16th, it was decided that the general meeting of 1892 be held at Chicago in order that an opportunity might be offered the western members of the Institute to participate in its proceedings. It was believed that the standing of the Institute as a national body would be enhanced by the holding of its meetings in other cities than New York, provided experience proved that this might be successfully done. The date was subsequently fixed for June 6th, 7th and 8th.

The total membership at the close of last year's report was 541, as follows:

Honorary	3
Members	161
Associate Members	377
Total	541
Associate Members elected during the year	111
Making a total of	652

The following have resigned during the year:

HENRY S. ISELIN,
THOS. B. KERR,
J. HOWARD PRATT, Jr.,
A. H. HENDERSON,
W. S. BELDING.
B. C. BATCHELLER,
H. C. ROOME,
W. P. TROWBRIDGE,
GRAHAM D. FITCH.

Total........................ 9.

We have lost by death:

GEORGE WORTHINGTON, of New York City,
CHARLES J. VAN DEPOELE, of Lynn, Mass.

Total........................ 2.

Elections cancelled by reason of failure to qualify	4
Suspended for non-payment of dues	5
Dropped from the list for non-payment of dues	17
Loss of membership	37

Deducting this loss of membership as stated leaves a remainder of 615, (a net gain of 74,) classified as follows:

Honorary Members	3
Members	179
Associate Members	433
Total membership May 1st, 1892	615

SECRETARY'S BALANCE SHEET.

FOR THE YEAR ENDING APRIL 30TH, 1892.

RALPH W. POPE, SECRETARY, in account with the
AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Dr.			*Cr.*
1892.			
To balance from 1891	$ 10 51	By Cash to Treasurer	$6,717 48
Sundry receipts	6 00		
Entrance fees	500 00		
Life Members	100 00		
Past dues	278 76		
Current dues	4,618 85		
Advance dues	144 17		
Electrotypes sold	528 31		
Typewriting and Stenography	217 14		
Transactions sold	223 15		
Subscriptions for Transactions	95 50		
For Binding	20 65	Secretary's Balance on hand	25 59
	$6,743 04		$6,743 04

DISBURSEMENTS DURING THE YEAR.

The Treasurer has disbursed upon warrants drawn by the Secretary, approved by Council and Finance Committee the amount of $7,224.89 classified as follows:

Special Deposit Mercantile Trust Co	$ 500 00
Stenography and Typewriting	556 41
Stationery and Miscellaneous Printing	505 45
Postage	299 95
Messenger Service	40 27
Salaries	1,441 67
Engraving and Electrotyping	649 14
Meeting Expenses	480 10
Rent	600 00
Printing Transactions	1,801 08
Express	4 60
Telegrams	3 61
Binding	170 52
Office Fittings	15 38
Office Expenses	75 46
Copyright	4 50
Duties	3 00
Extra Clerical Work	50 00
Library	3 75
Engrossing	20 00
	$7,224 89

This amount should be credited with the following items:

Special Deposit	$ 500 00	
Electrotypes Sold	528 31	
Received for Typewriting and Stenography	217 74	
Received for Binding	20 65	
Sundry Receipts	6 00	
	$1,272 70	1,272 70
Showing the net expenses of the year to have been		$5,952 19
And the net receipts for fees, dues, sales and subscription on account of Transactions		5,960 43
Showing an excess of net receipts over net expenses of		8 24
The outstanding current bills against the Institution amount to		491 17
And the amount of uncollected bills from other than members		$ 305 94

The arrearages of dues for the past year amount to $480.00 most of which is probably collectible.

In accordance with the instructions of the last annual meeting, the Secretary and the Treasurer acting as Trustees, made a special deposit of the $350.00, previously reported as a building fund, also $500.00 surplus from the General Fund making a total of $850.00 which has drawn interest at the rate of 3 per cent. This rate was reduced to 2 per cent., May 14th. The Treasurer's cash balance on April 30th was $245.53, and the Secretary's cash balance the same date $25.56, making the current balance carried forward to the next year $271 09. Special deposit $850.00.

Respectfully submitted by direction of Council,

RALPH W. POPE, *Secretary.*

New York, May 1, 1892.

TREASURER'S REPORT,

FROM MAY 15 1891 TO APRIL 30, 1892.

GEORGE M. PHELPS, TREASURER, in account with

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Dr.

Balance May 16, 1891, from previous year	$ 752 94	
Received from Secretary, May 16, 1891 to April 30. 1892	6,717 48	$7,470 42

Cr.

Payments, from May 16, 1994 to April 30, 1892, on warrants from Secretary, Nos. 267 to 352, inclusive	$7,224 89	
Balance to new account	245 53	$7,470 42
Balance on hand, General Fund, May 1, 1892	$ 245 53	

BUILDING FUND.

Balance on hand, May 16, 1891	$ 350 00
Transferred from General Fund, July 1, 1892	500 00
	$ 850 00
Total General Fund and Building Fund	$1,095 53

N. B.—Warrant No. 286, for the transfer of Building Fund to the Mercantile Trust Company on certificate of deposit, was for $850.00, including $350.00 already entered in Special Account, and $500,00 from General Fund. The General Fund is therefore credited with $500 00 only—on Warrant No 286.

Respectfully submitted,

GEORGE M. PHELPS, *Treasurer.*

The Auditing Committee after examining the accounts and vouchers, and comparing them with the above statements, reported that they had found them correct, and it was voted that they be accepted and placed on file.

The Chair appointed Messrs. Francis R. Upton, George H. Stockbridge and Dr. Cary T. Hutchinson an Auditing Committee to examine the books and reports.

The following officers were elected to fill vacancies occurring under the Rules:

President,
Frank J. Sprague.
Term Expires, 1893.

Vice-Presidents,
(Terms Expire 1894.)
A. E. Kennelly,
Nikola Tesla,
Oscar T. Crosby.

Managers,
(Terms Expire 1895.)
Charles Wirt,
Angus S. Hibbard,
Michael I. Pupin,
Charles P. Steinmetz.

Treasurer,
George M. Phelps.

Secretary,
Ralph W. Pope.

Terms Expire 1893

On motion of Mr. Upton, it was voted that the Trustees be authorized to set aside $150 from the General Fund during the coming year at their discretion adding it to the present building fund of $1000, and investing it at a more remunerative rate if possible.

On motion of Mr. T. C. Martin, a committee of three was appointed to revise the present method of electing officers, and to report at the General Meeting at Chicago in June.

The Chair appointed Messrs. T. C. Martin and P. V. R. Van Wyck, Jr., a committee to escort the President elect to the Chair. On assuming the Chair, President Sprague said:

Gentlemen of the Institute,—As you have already been informed, this honor has come to me suddenly, and I presume I may say, following the dictates of political practice, entirely unsought. I have to thank you very much for the confidence which you have expressed by the vote. I feel, however, with Mr. Martin that the method of casting the vote which was employed to-day is not one which necessarily shows the real sentiments of this association. We have an organization which is truly representative of the electrical engineers of the United States. Not over a quarter of them are residents of this city, and not more than a quarter, and in fact much less than that can ordinarily be present at these meetings, hence I am heartily in accord with the suggestion that has been made that there should be a more representative method of ascertaining who is really the choice, not only of those present but who shall be the representative of the more sober and carefully considered judgment of this association. However, having been elected, I shall try to do all in my power for the benefit of the Institute of Electrical Engineers. We have a year before us which is going to mark the inauguration of the greatest electrical exhibition which has ever been held and which probably will be held for a number of years. It is one in which the intelligence and skill and all that has made American electrical engineering a success, are going to be brought in competition with the best that the old country can bring us. There is now an Advisory Council whose duty is to aid the World's Fair authorities in getting together a representative congress of the electricians of the world. The officers and members of this Institute will have much to do in making that a representative body. Before this year closes it is probable that there will be advances in electrical engineering which are now merely suggested. I believe I am justified in saying that the year will be marked perhaps, as no year in the past has ever been by great works, and on that account, it gives me great pleasure to preside over the deliberations of this Institute.

In thanking the Institute for the confidence shown by my election, I wish to move a vote of thanks to the Vice-President who has just vacated his Chair, for the very able manner in which he has performed the duties of the office, and especially in view of the great amount of work which has fallen to his particular sphere in the absence of the Past-President. I am sure all those who are acquainted with Mr. Lockwood, with his intimate connection with one of the greatest industries, and also with his independent position as an electrical engineer, all in fact who know his ability, energy and honesty of purpose, will heartily agree with me that we can do no more graceful thing than to extend a vote of thanks to him for his past services.

Mr. Martin:—I move that we make that a rising vote.

Mr. Sprague's motion was carried by a rising vote.

Mr. Lockwood:—Mr. President and Gentlemen of the Institute: I am already, what I hardly expected to be until I had finished dinner, too full for utterance. I feel deeply the appreciative vote which has just been tendered to me. I am very glad indeed if I have been able to be of any service to the Institute. It has been my fortune, as I said at the beginning of the past year, to have in some measure to stand in the boots of another man, and I have endeavored, although my understanding is perhaps not as great as his, to fill them. But whatever success I may have had by the aid of the Council, to whom collectively I wish to express my hearty thanks—whatever good I have been enabled to do by the very efficient aid of the Secretary, I can only say has been due to hard work, for I am a most thorough believer in the phrase which is frequently, and erroneously I believe, attributed to Talleyrand, that genius is simply an infinite capacity for taking pains At a very early stage in my history, which has been somewhat of a vareigated character, I made up my mind that the only genius I possessed, was that kind of genius, and I endeavored to make it as available as possible to myself and to others. I have believed all my life that whatever is worth doing is worth doing as well as may be, and therefore I have endeavored to serve the Institute to the best of my power. I thank you for your very kind vote.

The Secretary read the following list of Associate Members elected and transferred at Council Meeting this date:

Name.	Address.	Endorsed by.
Beals, Pascal P.	Electrical Contractor and Agent for the Waddell-Entz Electric Co., 50 Terrace, Buffalo, N. Y.	Justus B. Entz. Montgomery Waddell. Thos. H. Foote.
Berthold, Victor M.	With T. D. Lockwood in Patent Department of American Bell Telephone Co., 16 Upton Street, Cambridgeport, Mass.	Chas. R. Cross. Thos. D. Lockwood. I. H. Farnham.
Bosson, Frederick N.	Electrician, Calumet and Hecla Mining Co., Calumet, Mich.	H. Ward Leonard. George Cutter. R. W. Pope.
Floy, Henry	Graduate Student in Electrical Engineering, Cornell University, Barnes Hall, Ithaca, N. Y.	Ernest Merritt. Edw. L. Nichols. Harris J. Ryan.
Gross, S. Ross	Electrician, Tennessee Coal, Iron & R. R. Co., Ensley, Ala.	Henry B. Cram. Walter C. Fish. S. C. Peck.
Little, Franklin P.	Manager, F. P. Little & Co., 141 East Seneca St., Buffalo, N. Y.	H. A. Foster. S. S. Wheeler. Geo. M. Phelps.
McRae, Austin Lee	Professor of Physics, Missouri School of Mines, Rolla, Mo.	Edwin H. Hall. H. V. Hayes. Brown Ayres.
Perry, Nelson W., E. M.	Editor, *Electricity*, 6 Park Place, New York City, Residence, 40 Sidney Place, Brooklyn, N. Y.	H. L. Webb. H. Ward Leonard. R. W. Pope.
Shaw, Edwin C.	General Superintendent, Thomson-Houston E. L. and P. Co., 40 Court St., Buffalo, N. Y.	H. A. Foster. A. E. Woolf. A. E. Winchester.
Verley, Horace S. L.	With Dr. Wm. E. Geyer, as Laboratory Assistant, Stevens Institute, Hoboken, N. J.	Wm. E. Geyer. J. H. Cuntz Joseph Wetzler.

Total 10.

Elected to Honorary Membership, by Council, May 17th, 1892.

Field, Cyrus W.	Gramercy Park, New York City.
Kelvin, *Lord*, LL.D., F.R.SS. L. and E.	The University, Glasgow, Scotland.
Siemens, Dr., Werner von,	Berlinerstrasse 36, Charlottenburg, Germany.

Transferred from associate to full membership.

Approved by Board of Examiners, March 14th, 1892.

Bayles, Robert N.	Electrician, The "C & C" Electric Motor Co., New York City.
Gray, Elisha	Highland Park, Ill.
Hewitt, Charles	The Edison General Electric Co., New York City.
Inrig, Alec. G.	Electrical Engineer, Antwerp, Belgium.
MacFarlane, Alexander	Professor of Physics, University of Texas, Austin, Texas.

Total, 5.

The President appointed Messrs. F. B. Herzog, Francis R. Upton and T. C. Martin as a committee on revision of the method of electing officers to report at the General Meeting at Chicago.

The annual meeting then adjourned and the members reassembled at "The Arena," No. 41 West 31st street, where a dinner was served, at which 65 members and guests were present. President Sprague presided, and remarks were made by Thos. D. Lockwood, Oscar T. Crosby, Charles E. Emery, Oberlin Smith, Prof. George Forbes, Dr. Louis Duncan and Past-Presidents T. C. Martin and Franklin L. Pope.

This was the first purely social gathering of the Institute, and was so thoroughly enjoyed that it was voted to organize a similar event annually hereafter.

OBITUARY.

Charles Joseph Van Depoele was born in the Province of West Flanders, Belgium, in the year 1846. His birthplace was the small place of Litchervelde, situated about sixty miles northwest of the city of Brussels. He died in Lynn, Massachusetts, March 18th, 1892. His death occurred after a sickness of four months, and it was, primarily, due to heart failure. Mr. Van Depoele was elected an Associate Member of the Institute, June 5th, 1888, and transferred to full membership September 7th, 1889.

When about ten years old young Van Depoele saw the telegraph system of the East Flanders railway installed, and as his father was master mechanic of the road he had an opportunity of studying into the mysteries of the working of the telegraph, which he was not slow in doing as he had early acquired a taste for natural philosophy, and read with avidity all works within reach. His taste for machinery was, undoubtedly, inherited from his father and his familiarity with the use of tools was of great advantage to him in his early work. We find him at the age of fifteen or sixteen years building telegraph instruments, Ruhmkorff coils, and going so far as to construct a Bunsen battery of forty cells. In this work he did not receive the encouragement of his father, who was very much opposed to his son spending his spare time in this way. His father apprenticed him to a cabinet-maker and he mastered the trade in a remarkably short time.

In 1864 his father moved to Lille, France, where young Van Depoele became interested in sculpture and worked for some time in the works of Buisine–Rigot, where more than two hundred sculptors were employed. He distinguished himself by making an immense piece of carved work some eighty feet in height, an altar-piece for a large cathedral. During this time, however, his spare moments were spent either reading electrical books or building and experimenting with electrical apparatus. His father strongly disapproved, and thought his time and labor might be better spent.

When about twenty-three years old he decided to come to America, and he arrived in Detroit in the summer of 1869. He immediately engaged in the church furniture business, but the success of this venture did not make him forget or lose interest in his electrical experiments, but the income allowed him to begin experiments of greater magnitude than he had hitherto attempted. By the aid of a large Bunsen battery he began experiments with arc lamps, and later when the description of the Gramme machine was published he built a model of it. Outside of lighting his workshop, his first commercial work was done in 1878 when he furnished arc lights for Forepaugh's Circus, at Campas Park, using for this purpose a three light dynamo. The front of the Detroit Opera House was lighted by him during the same year. This practical work led directly to the organization of a stock company with a capital of $125,000. The company immediately started the manufacture of electric light apparatus at Nos. 29 and 31 Atwater street, and they soon had a number of successful plants in operation on excursion boats running on the Detroit River.

About this time he began to pay attention to electricity as a motive power, and during the year 1880 did a great deal of experimenting. He experimented with pieces of carbon for collecting the current from the commutator of dynamos. During the latter part of the year 1880 the company was reorganized with a capital stock of $500,000, and the works moved to Chicago where large arc lighting machines were built and his first experiments were made in the propulsion of street cars. His work in this direction at the Chicago Exposition and at the Exposition at Toronto, Canada, are familiar to all, and although the courts have not yet decided on the patent situation as to who the pioneer in electric matters was, yet it would seem that Mr. Van Depoele's claim can be substantiated.

Mr. Van Depoele had a most wonderful capacity for hard work, and no matter what reverses came he was always hopeful, always ready and willing to carry on his work at all hours of the day or night. Perhaps the best measure of his industry can be made by referring to his record in the Patent Office. We find that his first electrical patent was for an arc lamp and was granted April 27th, 1880, the application being filed September 20th, 1879, and from that time until August, 1887, he secured ninety-one patents of which the majority related to electric lighting apparatus. arc and incandescent lamps, dynamo electric generators, motors, converters and current controlling apparatus, together with a large number of patents relating to electric railways, and also to reciprocating electric engines and that form of elevated railways known as "telpher systems."

In 1883 he received one of the earliest patents on electric railway underground conduit systems, and in February of 1886 he patented the overhead trolley switch now in use all over the country. During the years 1888 and 1890 (after he had become associated with the Thomson–Houson Electric Company) Mr. Van Depoele applied for and received fifty-three patents beginning with the universally used carbon commutator brush and including five different systems of underground conduit transmission together with the numerous devices entering into the now well-known overhead trolley system construction. In addition to these the list includes the pioneer patents on the closed circuit reciprocating electric percussion drill. During the year 1890, forty-six patents were granted him, including overhead electric systems, systems employing converters for modifying the current to be employed in electric railway circuits, contact devices for electric systems, telpher systems; several forms of electric railway underground conduits in which the conduit is entirely closed, and a number of methods of adapting electric currents to the propulsion of reciprocating engines, also several patents for alternating current motors.

During the year 1891, thirty patents were granted, including electric railway systems, with overhead and underground conductors, several patents relating to alternating current motors, and a number relating to reciprocating electric engines. In the early part of the year 1892 a patent was granted for electric railways, the filing date of which was June, 1885, and which contains broad claims for the upward pressure overhead electric railway system. The last patent granted during his lifetime was filed in the early part of 1887 and was for a railway power station system which, although not thought much of by others in that year, has since been demonstrated to be most important. The entire number of patents granted to Chas. J. Van Depoele is 225, and at the time of his death there were seventy-one applications pending before the U. S. Patent Office in his name.

The list is a long one and represents many years of diligent study and earnest effort to promote electrical science in all its branches. Whilst it is impracticable to enumerate in detail, it may be said that the patent record left by Mr. Van Depoele has been equalled by only a few of his competitors and excelled by none in the scope and variety of the subjects treated by him. His range of vision, quickness of perception, and fertility of resource, were something marvellous, and when it is considered that he began experimenting in a small private laboratory in the city of Detroit in 1872, when, to obtain insulated wire of any description was a matter of personal effort, and when it is considered that in the summer of 1883 he had actually tested and for sale a dynamo electric machine operating 60 lights, it will be seen that he did pioneer work in everything which he undertook, and that in all probability he contributed as much to the development of the electric industry as any other one man.

The Ninth General Meeting of the Institute was held at Chicago, Ill., June 6th, 7th and 8th. The opening session was held at the Grand Pacific Hotel, the meeting being called to order by President Sprague, who then relinquished the Chair to Vice-President Lockwood. In the absence of the authors the following paper was read by Vice-President Hering:

A paper read at the General Meeting of the American Institute of Electrical Engineers, Chicago, Ill., June 6th, 1892, Vice-President Lockwood in the Chair.

NOTE ON SOME EXPERIMENTS WITH ALTERNATING CURRENTS.

BY DR. LOUIS DUNCAN,

Assisted by Mr. E. R. Carichoff and Messrs. R. H. and G. E Hutton.

I must apologize for the brevity and scope of this paper, not because I sympathize with long papers, but because I had intended to embody in this the results of a considerable amount of work done during the year, and I find that the rush of work at the last of our term has made it impossible for me to do more than write a note descriptive of the methods used, and to give a few illustrations of its use. Last fall I published a preliminary statement of a method for obtaining alternating current curves, and it has proved so satisfactory that we have used it in a number of investigations of alternating current phenomena.

The first real representation of an alternating current curve was made by Joubert, who employed a contact piece rotating with the armature of a dynamo, touching a fixed brush at a point of the revolution determined by the position of the brush with relation to the dynamo poles. He obtained a curve for current which was approximately a sine curve. Later, in 1888, Messrs Wilkes, Hutchinson and myself modified the method to obtain the curves for E. M. F. and current in the primaries and secondaries of induction coils. This modification was used afterward at Cornell, where Professor Ryan and others made valuable experiments, and it has been more or less generally employed. The principal difficulties lie in the fact that with a single commutating arrangement but one curve can be taken at a time, and as an experiment involving four curves takes something like 45 minutes, the condition may seriously change; while the whole voltage of the dynamo is at times on the commutator. It is at best rather a tedious method. In our work referred to above, and in the work at Cornell, E. M. F's. were measured by an electrometer, and currents were obtained by getting potential differences at the terminals of a non-inductive resistance through which the current passed. Lately, in France, two commutating arrangements have been used to give two curves at once, with a condenser charged through the contact and discharged through a d'Arsonval galvanometer.

By the method which I described last fall, any number of curves may be obtained simultaneously, with only a few volts on the commutator and with a rapidity and accuracy much greater than by the other methods. Briefly, a number of dynamometers are used, equal to the number of simultaneous curves to be obtained. These dynamometers are very cheap and convenient. The first ones used were made by Mr. Carichoff and myself, and were quite satisfactory, but experience having suggested some improvement, and our time being fully occupied, Messrs. E. S. Ritchie and Sons made four of them, such as are shown in the picture [Fig. 1.] I will describe them more minutely below.

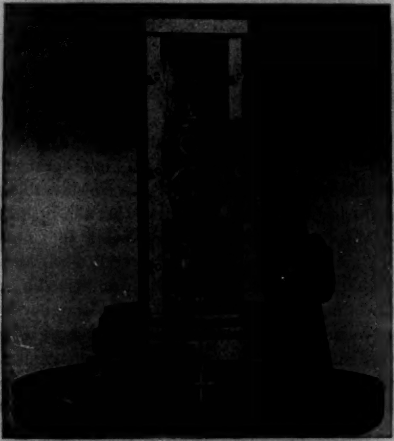
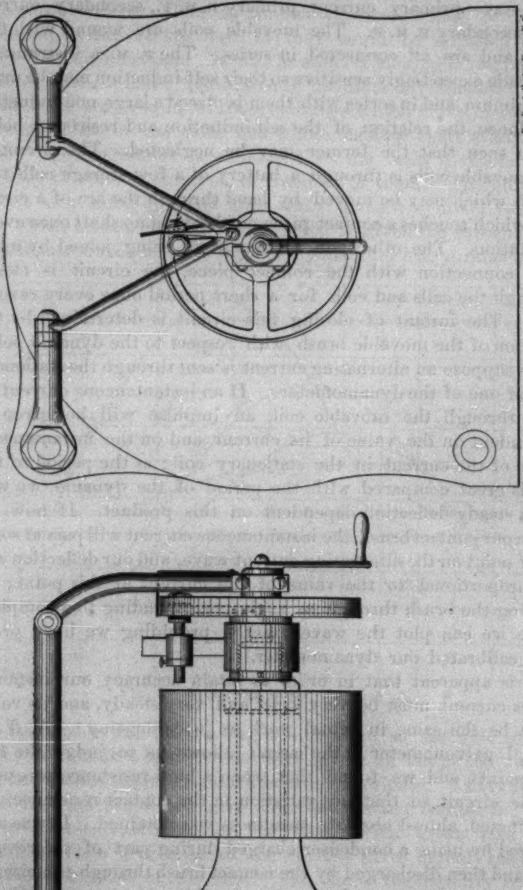


FIG. 1.

The four dynamometers have their stationary coils wound



Bradley & Poates Engr's, N.Y.

FIG. 2.

for,—say—primary current, primary E. M. F., secondary current and secondary E. M. F. The movable coils are wound with fine wire, and are all connected in series. The E. M. F. instruments are made exceedingly sensitive so their self-induction may be made a minimum, and in series with them is placed a large non-inductive resistance, the relation of the self-induction and resistance being made such that the former may be neglected. The circuit of the movable coils is through a battery of a few storage cells to a brush which may be moved by hand through the arc of a circle, and which touches a contact piece on the dynamo shaft once every revolution. The other end of the circuit being joined by a sliding connection with the contact piece, the circuit is closed through the cells and coils for a short period once every revolution. The instant of closing this circuit is determined by the position of the movable brush with respect to the dynamo poles. Now suppose an alternating current is sent through the stationary coil of one of the dynamometers. If an instantaneous current be sent through the movable coil, an impulse will be given it dependent on the value of its current and on the instantaneous value of the current in the stationary coil; as the period of the coil is great compared with the period of the dynamo, we will get a steady deflection dependent on this product. If now we move our contact brush, the instantaneous current will pass at some other point on the alternating current wave, and our deflection will be proportional to the value of the current at this point; by moving the brush through an angle corresponding to a complete wave we can plot the wave exactly, providing we have properly calibrated our dynamometer.

It is apparent that in order to attain accuracy our instantaneous current must be very brief and very steady, and its value must be the same in actual work as in calibrating. A d'Arsonval galvonometer in the circuit allows us to judge the two last points, and we found that when a high resistance was used in the circuit so that any variation in the contact resistance was eliminated, almost absolute steadiness was obtained. It was also assured by using a condenser charged during part of the revolution and then discharged by the contact brush through the circuit. To give an idea of the current in this circuit, an ordinary arrangement, when the condenser was not used, was 12 volts through 1000 ohms, and it must be remembered that this current only passes an exceedingly small part of the time.

Dynamometers:—The dynamometers were made of wood, mounted on a revolving base, so they could be adjusted with respect to the meridian. It will be seen on looking at the picture that one side is made removable to facilitate adjustments. The fixed coils are wound in two cylinders which move in and out through holes in the sides, and whose distance apart is adjusted when the instrument is calibrated, so the scale divisions read directly in volts or amperes, or fractions of them. The movable coil is suspended by a silk fibre, which gives it no directive force but which sustains it. Directive force is given the coil and current is introduced to it through two palladium hair springs which are fastened to stiff brass wires, which serve as the axis of the coil. A vane in a beaker filled with glycerine damps the swinging and makes the instrument absolutely aperiodic. I have rarely used instruments which give more satisfactory results, the zero is invariable, readings may be taken with great ease and rapidity, while a reversal of the current gives equal readings on the two sides of the zero. To show the rapidity of the readings, six curves, representing different quantities in a step-up-and-down transmission were taken in a little over five minutes, and in this case two instruments were used for four curves—those at the sending and receiving ends of the plant,—

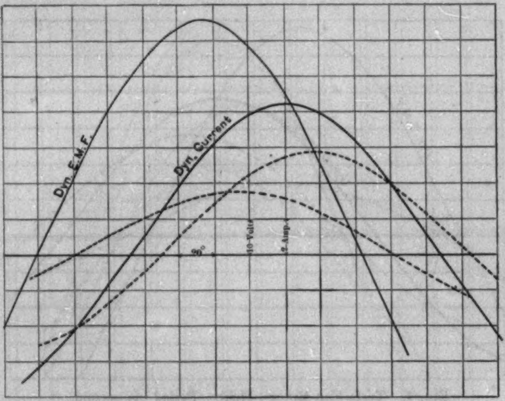


Fig. 3. Tesla Fan Motor. Period $\frac{1}{135}$.

and therefore had to be switched from one side to the other at every reading. Twenty points were usually taken for each wave. The curves could of course be photographed instead of observed, but the latter is so much simpler and more satisfactory that—especially in a laboratory where there are plenty of observers—I much prefer it.

Contact Arrangement: This is shown in Fig. 2. As it comes outside of the belt, and as it was often necessary to remove the latter, the swinging arrangement shown was designed and constructed by the Messrs. Hutton. On the shaft was a hard rubber cylinder with the brass contact almost flush with its surface. The whole arrangement worked admirably. The results of our work will have to be another story. I will simply give a few to illustrate the method.

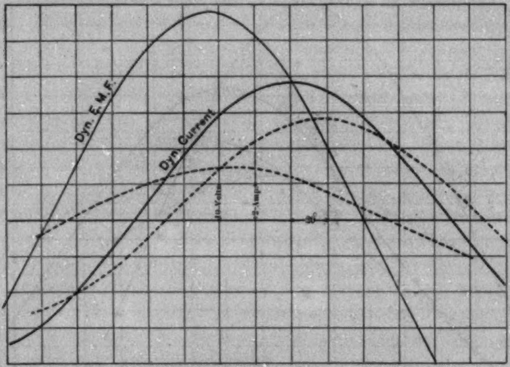


FIG. 4. Tesla Fan Motor—Held. Period $\frac{1}{160}$ second.

We have a small Tesla fan motor intended to work on an ordinary incandescent circuit. There are eight poles, the alternate ones having their windings in different circuits,—one set of poles having a few turns of coarse wire, the others having a greater number of fine wire turns. The curves in Fig. 3 show the condition of affairs when the motor was working; in Fig. 4 where the armature was held. In the second case the energy represented corresponds, of course, only to the losses. It seems to be the general impression by the way, that a rotating field motor starts up with the same torque as a corresponding continuous current machine. I have shown, however, that a two phase motor corresponds exactly to a converter,—or rather two converters,—in which the secondary has an outside resistance of $\frac{p^1}{p-p^1}$, where p^1 is the angular velocity of the armature and p that of the field. When p^1 is 0—that is when the motor is at rest,—it will be seen that the current is in the worst possible condition for starting and the torque for a given current is small. It is like starting a continuous current motor, with the brushes almost ninety degrees from the proper position.

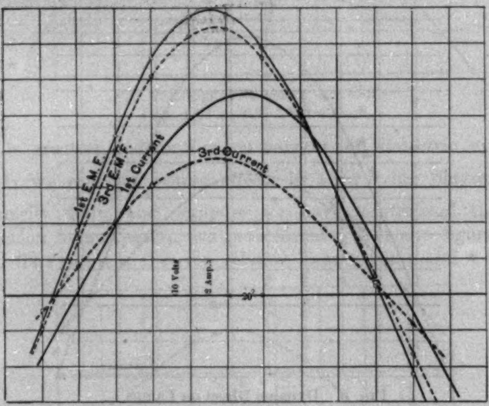


FIG. 5.

But most of our work has been with circuits in which both self-induction and capacity have been introduced. It is a well-known fact that circuits carrying alternating currents may be given the same period of vibration as the current, or to put it another way, circuits may be so arranged that with currents of a given period the sum of the energies stored up in the circuit and given out by it, is zero. With a condenser and self-induction properly adjusted, if energy from the current is being stored up in the condenser an equal quantity is being given to the current by virtue of the self-induction. That is, we can neutralize our self-induction by a capacity. I can hardly think of any fact more likely to play an important part in the future of alternating current work. For instance, if a circuit has in it a periodic current consisting of a number of sine curves of different periods—one-third, one-fifth, one-seventh, etc., the period of the fundamental wave; then if we put in inductive relation to this circuit another circuit whose period is—say—one-third that of the fundamental wave, this current will pick out the one-third period waves in the primary.

An ordinary sine curve E. M. F. may be made to give a periodic current wave with these higher harmonics in several ways—by having in the circuit a self-induction coil whose iron core is saturated when the current nears a maximum, or by using the same coil with a capacity which neutralizes one of the value of the varying self-induction; so we can produce these higher harmonics and we can pick them out. But it is a complex subject not to be treated here. In all such experiments however, there are several points which must be carefully attended to, or there will be very unsatisfactory results. If the iron core of the induction coil—provided such a core is used—is not properly laminated and annealed, or if there are considerable losses in the condenser the results will be disappointing. Take the case for example where there are eddy currents in the core and there is loss in the condenser. The first effect corresponds to a short-circuited secondary near the coil; the loss in the dielectric of the condenser means that energy

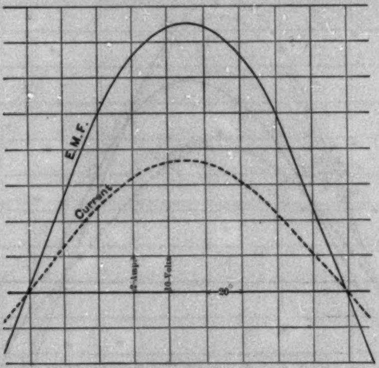


FIG. 6. Dynamo Direct on Lamps.

is taken from the current, and this energy must be represented by the product of the current into the E. M. F., e corresponding to the transfer of energy, or it is $e.\ i.$ The value of e is a maximum when the displacement is greatest,—that is when the current is greatest, and for a given current and dielectric varies as $\frac{1}{c.}$ If we represent these effects by a revolving diagram as is ordinarily done, and suppose we have neutralized the self-induction by a capacity, we get something like the figure [Fig. 12]. The result is that the relation between R. I. and E. instead of being 1 to 1 as would be the case were there no disturbing causes, is 1 to 1+. That is, for a given E. M. F. the current is less than it should be, and a glance at the figure will show that it is thrown ahead of the E. M. F. No possible change in our self-

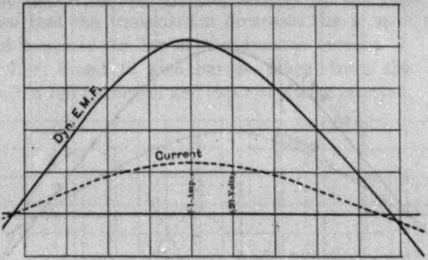


FIG. 7. Copper Condenser with Coils.
Cap. 1.35 M. F.'s. Wire Core.

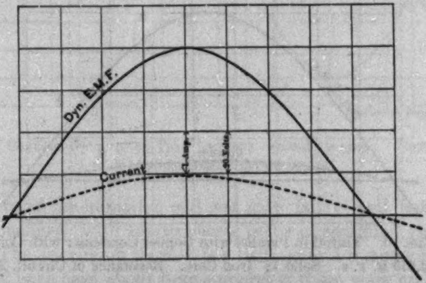


FIG. 8. Tinfoil Condenser with Coils.
Cap. 1.33 M. F.'s. Wire Core.

induction and capacity can remedy this decrease in the current, although we may, of course, make the phase angle zero if we wish.

An excellent way to make an iron core, by the way, is to shellac fine, well annealed wires, spread them along a piece of thin paper, brush them over with shellac and then roll the mat so formed into a cylinder. The losses in a condenser are usually small. They are discussed in a paper of Messrs. Hutin and Leblanc, published last spring in *La Lumiere Electrique*. We find however that if the condenser is made of very thin tinfoil and has a large area, then for heavy currents it may considerably decrease in capacity and offer a definite resistance to the current.

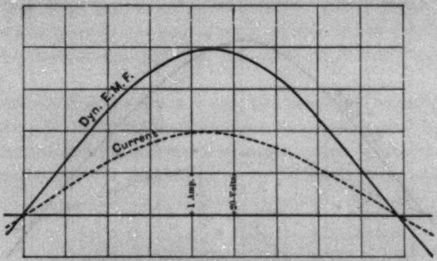


FIG. 9. Tinfoil in Parallel with Copper Condenser with Coils.
Cap. 2.675 M. F.'s. Wire Core.

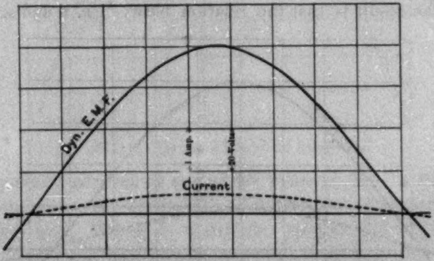


FIG. 10. Tinfoil in Parallel with Copper Condenser with Coils
Cap. 2.675 M. F.'s. Solid 1¾" Iron Core. Resistance of Circuit, 20.3 ω.

The curves given have been selected almost at random from a mass of data. We have not the time to discuss the results which are hardly within the scope of this paper. Figure 5 gives the curves at the two ends of a transmission plant where the voltage was raised from 50 to 1000 volts and reduced again. Fig. 6 gives the curves for the dynamo directly on the same lamps. It is seen that the transmission decreases the E. M. F. and current, and increases the lag of the dynamo current.

Figs. 7, 8, 9 and 10 give curves taken from the dynamo circuit when self-induction and capacities were inserted. In Fig. 7 the core of the induction coil was only fairly well laminated; and a condenser with copper sheets between paraffined paper was used. The maximum current is a little over 1.2 amperes, while it should have been about four amperes if there were no losses in the circuit. The second point of crossing is thrown out by a change of speed of the dynamo. Fig. 8 shows the result of substituting a tinfoil condenser of the same capacity in place of the copper one. In Fig. 9 both condensers were used and a considerable part of the wire core was withdrawn. In Fig. 10 with

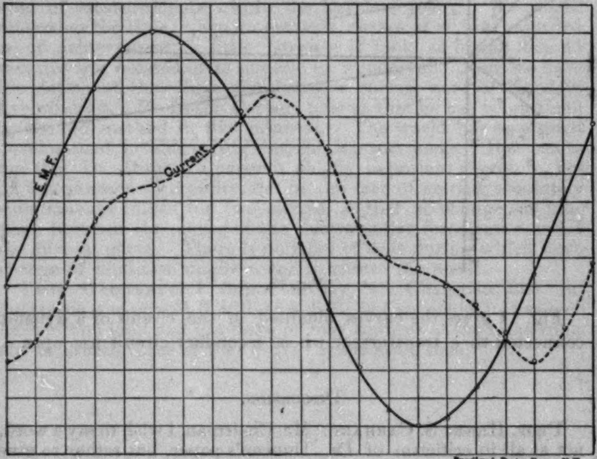


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FIG. 11. Siemens Dynamo on Transformer; secondary open. Curves taken with experimental dynamometers.

the same two condensers, a solid iron bar was used as the core of the induction coil.

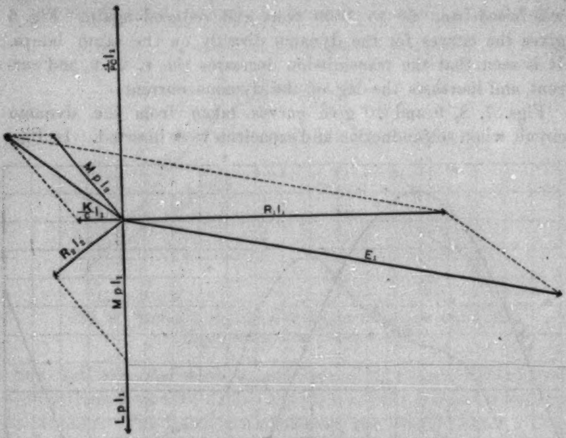

FIG. 12.

Fig. 11 gives the curves obtained in the circuit of a dynamo connected to a transformer whose secondary circuit was open.

DISCUSSION.

PROF. HENRY S. CARHART: Mr. Chairman, I wish to say a word, not at all in criticism of Dr. Duncan's paper, but rather to give some of my own experience in this matter. We have succeeded, by a very simple means, in getting these curves with only two observers, taking 20 points in five minutes. I have written off a few of the curves, but they are on a small scale. The apparatus is in general similar to that used in the older method, connected to the end of the shaft of the armature, with sliding contact. The instrument is well made and has worked perfectly from the start. The shunt is taken off from a single lamp of non-inductive resistance, 24 lamps across the primary. The current was 10,000 volts. I am not certain yet whether the lag between the current and the electromotive force comes out properly or not. In that case we use a galvanometer. In another instance the same apparatus is used, except that we use an electrometer. They came out better and faster, and are very satisfactory. The calibration was what I was about to speak of. An instrument made under precisely the same conditions with saturated paper, was put in place of the shunt around the lamp, and then run through the machine in exactly the same way, and through a commutating arrangement on the machine. We run this with precisely the same apparatus. Dr. Duncan's method very likely worked more rapidly, but this certainly answers very well for ordinary purposes.

Dr. Edward L. Nichols:—I should like to say, Mr. Chairman, that it seems to me that the paper, together with other work which we know of that is going on at the present time along these lines, goes to show what progress has been made in this kind of measurements within the last few years. We can all remember the time, when to get such curves at all was regarded as an achievement, a little later on it took an hour, then 45 minutes was boasted of as something wonderful. Now we have the statement that these simultaneous curves were obtained in five minutes. Professor Carhart tells us that he has taken them by the old method in five minutes. The world has seen great development in alternating current measurements. It seems to me that Dr. Duncan's paper is chiefly important therefore, not on the question of rapidity, but in the fact of getting absolutely simultaneous values for two curves, so that no change can take place between the taking of the electromotive force curve, andof the current curve. There is no lapse of time during which some change of condition might occur to vitiate the result.

Prof. Carhart: I forgot to say, Mr. Chairman, that our apparatus was provided with two disks, but we could just as well have put on four, and got three of four contacts at the same time, and measured three of four curves at the same time.

A paper read at the General Meeting of the American Institute of Electrical Engineers, Chicago, Ill., June 6th, 1892, Vice-President Lockwood in the Chair.

RATIONAL AND EMPIRICAL FORMULÆ SHOWING THE RELATION BETWEEN THE MAGNETO-MOTIVE FORCE (H) AND THE RESULTING MAGNETIZATION (B).

BY CHARLES E. EMERY, PH. D.

PART I.

SEC. 1. It is proposed to present in Part I of this paper the results of investigations designed to ascertain the general principles underlying the observed relations of the electric exciting or magneto-motive force (H) to the resulting magnetization (B), with a view of developing a rational equation therefor. It is believed that rational functions based on physical facts have been found ample to account for the peculiar shape of the H-B curves of magnetization. The theory that magnetization is the circulation of an ether is examined by the law of the flow of fluids, with the gratifying result that a rational fluid function has been developed which corresponds very well with the observed curves of magnetization and which, moreover, shows decidedly a phenomenon equivalent to that of magnetic saturation. The observed results indicate a more intense action during the earlier stages of magnetization than is provided by the fluid function, but the discussion shows that the theory of molecular magnets appears quite sufficient to account for such intensified action. Following the discussion on this subject there are presented in "Part II," Sec. 23, empirical formulæ for practical use, corresponding satisfactorily with typical H-B curves derived from experiments, and in the "Conclusion," Sec. 36, a general formulæ is derived, embodying the fluid function first discussed and a linear function, which together satisfactorily represent, on a rational basis, the observed conditions throughout the entire range of observation, including Ewing's experiments with very high magnetizations.

SEC. 2. The human mind must found its impressions of unseen phenomena on analogy, so it is natural to suppose that all "action at a distance" is due to the motion of something extending through that distance. The rational discussion is based on a theory which has heretofore been proposed in substance, though generally with hesitation, shown by the fact that other plausible theories were advanced in the same connection. It will be considered that the magnetic field results from the circulation along definite lines of an ethereal atmosphere of such extreme tenuity that it moves readily through all bodies, fluid or solid, of which our senses are cognizant, though like other fluids capable of being moved and directed by other forces and of being concentrated in certain paths by particular materials.

SEC. 3. If this theory be true the etheric fluid should respond to the same laws as govern the flow of other fluids, air and water for instance, and we will first examine the formulæ relating to fluids tangible directly to our senses, and see if the flow of a fluid of such extreme tenuity that we cannot feel, see or hear it, but only judge of its presence and its circulation by the results produced, will show in such formulæ a substantial agreement with the observed results. Starting with the well-known formula derived from the law of falling bodies,

$$v = \sqrt{2 g h} \tag{1}$$

in which evidently v equals the velocity, g the acceleratrix of gravity, and h the head or vertical height of column, we may derive at once the formula for the flow of fluid in a pipe or constrained space as follows:

SEC. 4. If $a =$ the area of the channel or pipe considered, $D =$ the specific weight or weight of a cubic unit of the fluid considered, and $G_1 =$ the weight of fluid passing a given point in a unit of time, we have

$$G_1 = a\, v\, D \tag{2}$$

By combination with Eq. 1 we have

$$G_1 = a\, D\, \sqrt{2 g h} \tag{3}$$

If p_0 equals the pressure per superficial unit, due to the head h, evidently

$$p_0 = D\,h \tag{4}$$

$$h = \frac{p_0}{D}, \text{ which, substituted in Eq. 3, gives} \tag{5}$$

$$G_1 = a\,\sqrt{2\,g\,D\,p_0} \tag{6}$$

which is the ordinary form of the equation showing the force required to overcome the inertia of the mass and give the same a velocity necessary to produce the flow G_1 in a circuit without resistance.

Sec. 5. There are various external conditions which also affect the flow, depending upon the apparatus and materials employed such as the particular shape of the nozzle through which the fluid is to flow and the "hydraulic mean depth" and the roughness of the surfaces of a pipe or channel. These differences are usually provided for by considering either that it requires a certain head h_1 or pressures p_1 to overcome such resistances, so the total pressure required will be $p_0 + p_1$. Generally in practice the force p_0 required to put the mass in motion is so small that it may be neglected in comparison with p_1 required to overcome resistances. It is found also that the flow does not vary directly as the pressure p_1, so a function of p_1, or a function of the velocity easily reduced to a function of p_1, is generally employed to develop a variable coefficient of p_1, which will provide for the varying external conditions. On this basis $p_1\,(f p_1)$ (read p_1 multiplied by function of p_1) is to be substituted for p_0 in Eq. 6.

Sec. 6. With an elastic fluid the density D practically varies with the total pressure, so if we let D_1 represent the weight of a cubic unit at unit pressure, the weight due to any other pressure p_2 will be $p_2 D_1$. which is to be substituted for D in Eq. 6. Thus we see that for an elastic fluid the flow depends upon two variables, viz.: the density and the pressure required to overcome the resistances. Many formulæ have been developed to show the observed relations, some of which are very imperfect, except when applied to the observations upon which they were based, while others fairly express the true relation. Many years ago Napier advanced the theory, based on experiments with the discharge of steam through orifices, that the flow increased rapidly with the difference of pressure at the two ends of the orifice until the difference of pressure somewhat exceeded one-half the total pressure, when the flow abruptly became constant for all increased differences of pressure. During the construction of the plant of the New York Steam Company the writer had a series of experiments carefully made on the subject, and a smooth curve was obtained with limiting values such as indicated by Napier. In other words, the flow increased much more rapidly than the difference in pressure at first, and slower afterward, as if approaching a limiting constant value.

SEC. 7. The writer has obtained a rational function expressing these relations in the following manner which has not been heretofore published. If p be the total pressure, it is evidently equal to $p_1 + p_2$ above, viz.: to the sum of the pressures required to overcome resistance and available to increase the density. In any given case p is constant and p_1 and p_2 form different proportions of p. If, therefore, we consider that the portion p_1 required to overcome resistances $= r p$ the remaining portion p_2 to which the density is proportioned will equal $(1 - r) p$. The expression $p_1 (f p_1)$ (Sec. 5) may be replaced by $r (f p) p$ in which the coefficient of p has two factors r and $(f p)$. So also the density (Sec. 6) $p_2 \; D_1 = D_1 (1 - r) p$. The product of the two expressions is then $D_1 r (1 - r) (f p) p^2$. All the considerations above expressed may be embodied in an equation, and the difficulties some have encountered overcome by considering that Eq. 6, relating to the flow of an incompressible fluid, indicates the general form of a *differential* equation of the flow of an elastic fluid which will show the momentary rate of flow of its elementary masses. Calling G the weight of elastic fluid passing a given point in a given time and substituting the same for G_1 and the expression above for $D \, p_0$ in Eq. 6, squaring the result temporarily and expressing it as a differential, we have for the differential equation of the square of the flow of an elastic fluid the following:

$$(7) \qquad d \, G_2 = 2 \, a^2 \, g \, D_1 \, p^2 \, (f \, p) \, (1 - r) \, r \, d \, r$$

Integrating and taking the root we have

$$(8) \qquad G = \left(2 \, a^2 g \, D_1 \, \left(\frac{r^2}{2} - \frac{r^3}{3}\right) (f p) \; p^2_{max}\right)^{½}$$

$$\text{Making} \quad \left(\frac{2 \, a^2 \, g \, D_1}{6}\right)^{½} = A, \text{ we have}$$

$$(9) \qquad G = A \, (3 \, r^2 - 2 \, r^3)^{½} \, (f \, p) \, p_{max}$$

In applying this equation to etheric magnetic flow, it should be borne in mind that Ewing shows that the magnetization B is made up of two parts, one due to what we call the concentrating influence of the iron, which in connection with a somewhat different course of reasoning he calls I, and to which Eq. (9) can only be made to apply, and the other directly proportioned to H, due to what we describe herein as the flow of lines through the space occupied by the iron as though no iron were present. For etheric magnetic flow, then, the above equation takes the form

(10) $$I = A\,(3\,r^2 - 2\,r^3)^{1/2}\,(f\,H)\;H_s$$

in which H_s = the value of H when the iron is saturated. We have also

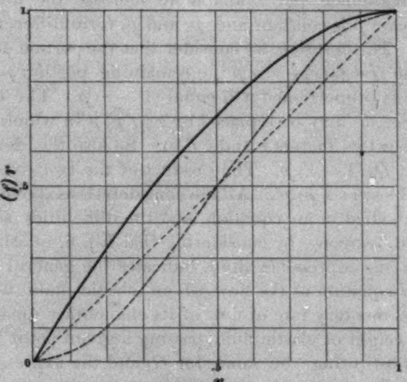

Fig. 1.

(10a) $$B = (H + I) = H + A\,(3\,r^2 - 2\,r^3)^{1/2}\,(f\,H)\;H_s$$

in which the function $(3\,r_2 - 2\,r_3)^{1/2}$ develops a factor of the variable coefficient of H_s, depending upon the law of the flow of fluids, and all influences on the value of I, due to external causes, are provided for by the other factor, $A\,(f\,H)$.

Sec. 8. In the expression $\sqrt{3\,r^2 - 2\,r^3}$, if $r = 0$, so that no part of the pressure is expended to produce motion, the whole equation reduces to 0 and there is no flow. If $r = 1$ the expression containing r reduces to unity. The ordinates of the fluid function are shown proportionally in Fig. 1. A direct linear function is shown by the diagonal. The modification due to $3r^2 - 2r^3$ is represented by the reversed curve in dotted lines crossing the diagonal at the center of the figure, and the complete function including the square root of the above is shown by the heavy line curve. The latter is in general similar to a curve showing the relation of the exciting force H to Ewing's intensity of magnetization I, and also resembles an H-B curve of magnetization, with a less intense initial action, which is yet to be provided for by substituting in Eq. (10a.) the value of $(f\,H)$. In Part II a number of H-B curves are shown, which may be examined for comparison.

Sec. 9. If we adopt the theory of etheric flow, we must conceive that the magnetization expressed at present as a definite number of magnetic lines is represented by the flow of a definite weight of etheric fluid. Recalling the signification of r and $1 - r$ in connection with Eq. (7), it will be seen that the fluid function simply shows that when r is small, but little of the exciting force is expended in forcing forward the etheric fluid and the larger portion of such force is utilized in increasing the density of the etheric fluid circulated, so that the weight circulated, and therefore utilized, is large compared to the exciting force expended to produce circulation, a higher percentage of the total exciting force is utilized and a higher efficiency of transmission obtained. When, however, the flow in a given area is increased, a larger proportion of the exciting force is required to produce the circulation and a smaller quantity available to increase the density, so the weight moved approaches a limiting or constant value which evidently will be reached when the volume is infinite and the density zero, a condition typified when $r = 1$ in Eq. (0) and Fig. 1, and by the magnetic condition that the permeability μ and induction I, due to the iron, each reach a maximum. *We therefore call especial attention to the fact that the simple laws relating to the flow of tangible fluids show a limiting condition analogous to that of magnetic saturation.* If as in Sec. 44, the value of $(f\,H)$ in Eq. (10) be derived by modifying the exponent of the fluid function, evidently at saturation, from Eq. (10.) $I_s = AH_s =$ also K Sec. 42. and from Eq. (10a), $B_s = (A+1)\,H_s$.

Sec. 10. The fluid function not only has an important bearing on the H-B curve of magnetization, as the shape of the curve would indicate, but may also, it is thought, be applied to correct on a rational basis the laws of the magnetic circuit. Recurring to formula 10 and to curves shown in Fig. 1, it is evident that, for a circuit of low resistance, very small values of r apply. When there are air gaps in the circuit increasing the resistance there should by analogy be a high pressure developed in the portion of the iron excited and a great fall of pressure at the gaps producing the principal resistance. This would require higher values of r, the same as if more etheric fluid were to be forced through a circuit of low resistance, but the actual flow or flux through the circuit containing the resistance would be less than in the other case, and this would of itself cause the resistance in the excited portion of the circuit to be less and permit a higher pressure there than if the same weight of etheric fluid (number of magnetic lines) were flowing at the time as would flow with the same exciting force in a circuit of low resistance. These considerations show that the laws of etheric flow in a magnetic circuit of low resistance need modification when applied to a circuit provided with air gaps. The fluid function above developed should aid in making the necessary corrections. The whole subject is so broad that this branch cannot at this time be further considered.

SEC. 11. It is apparent that the changes of the coefficient of H due to the fluid function alone are insufficient to account for the extreme values and abrupt changes in H-B curves of magnetization. We, however, find that not only do the laws relating to the flow of elastic fluids impose no condition antagonistic to the adoption of the theory of etheric flow, but on the other hand the elements of the fluid function vary in the right direction, and show a limiting value akin to magnetic saturation. They therefore aid in the development of the H-B curve, and in addition indicate means whereby one general law may be extended from the excitation of an iron ring of low magnetic resistance to magnetic circuits of the highest resistance.

SEC. 12. As mentioned above, the shape of nozzles, the hydraulic mean depth and the roughness of pipes and natural channels modify the flow of tangible fluids, as represented by $(f\,p)$ in Eq. (9), so in investigating etheric flow the conditions of the problem other than those imposed by the fluid function must be based on external conditions due to the environment represented by $(f\,H)$ in Eq. (10), and one of the elements of the H-B

curve of magnetization be derived not from the laws relating to the flow of fluids, but from the peculiarities of the material used for the concentration and transmission of the etheric current (magnetic lines of force), iron for instance, as modified by the reactions between the flowing current and the molecules of the iron.

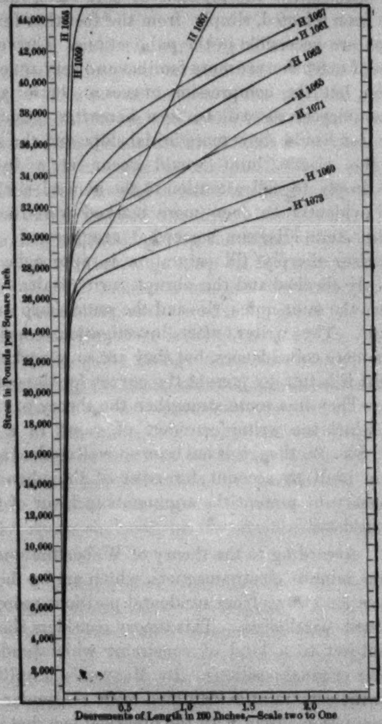

Fig. 2.

Sec. 13. At this point special attention is directed to Fig. 2, which is a reproduction of one of Kirkaldy's sheets, showing the decrements of length, under a gradually increasing thrusting stress, of ten Fagersta steel plates, each 100 inches long, part of which were annealed. The general similarity of appearance between the several curves and the typical *H-B* curve, Fig. 3, will be at once apparent. Curves showing the results arising from the mechanical compression of soft steel, rather than of iron, have been selected, simply from the fact that few compression curves are accessible in the publications. Curves showing increments of extension are more familiar and very much resemble those given, but the compression curves are better adapted for the present purpose, as will be seen hereafter. Corresponding curves for iron would show more initial slope and the sharp bend marking the elastic limit would occur at a lower strain. We wish simply to call attention to the general similarity, and this would evidently be even more marked were the horizontal scale of the strain diagram somewhat exaggerated. The magnetization rises sharply till saturation is approached, when its rise is quickly checked and the abrupt turn results. The strain curve shows the same quick rise and the same sharp turn at the elastic limit. The writer, after investigation, considers these similarities mere coincidences, but they are so remarkable that he has thought it better to present the curves for the consideration of others. They in a sense strengthen the theory of "molecular magnets," which the writer considers of value in a subsidiary sense only, Sec. 19, though it has been so well studied as to appear sufficient in itself to account for most of the phenomena. It seems necessary to present the arguments in favor of this theory somewhat in detail.

SEC. 14. According to the theory of Weber the molecules of iron become minute electro-magnets, which are by the operation of magnetization swung from accidental positions more and more nearly toward parallelism. This theory considers that the magnets are subject to a kind of constraint which tends to return them to their original positions. In Maxwell's amplification of the theory, the constraint is identified with molecular attraction, and this assumption agrees with the observed fact that if a moderate exciting force be applied and removed the magnets will swing back to their original positions, and the extent of the motion be sensibly proportioned to the force applied. Ewing claims that reaction among the elementary magnets themselves is sufficient to account for the phenomena, or, as understood by the writer, that on the withdrawal of the exciting force the original magnetic condition reasserts itself.[1] This theory would seem to be insufficient to account for the great magnetic efficiency of the softest iron which retains no trace of permanent magnetism. All of the modifications appear to have value, but the reactions due to molecular attraction, considered by Maxwell, are considered of the greatest importance.

SEC. 15. The strain diagram, Fig. 2, appears to confirm the theory of molecular magnets as modified by Maxwell, as it can be said that in one case we have a given material subjected to external mechanical forces, and in the other to the internal forces of magnetism; that the molecular attraction overcome in swinging the molecular magnets towards parallelism is represented by the strength of the material to resist crushing and tensible forces, and that the elastic reaction due thereto measures the intensity of the possible magnetization during the initial stages, shown by the curve of magnetization, Fig. 3, up to the abrupt turn. At or about that time, the molecular magnets have reached nearly the condition of parallelism, and are offering great resistance to further motion, and this effect is heightened by the fact that the several molecules are being attracted toward each other, actually shortening the mass and making further motion more difficult, so that after the approach to saturation, shown by the rapid bend in the curve, Fig. 3, the only additional magnetization which can be received by the molecules is that due to their still closer approximation to parallelism, and to the results must be added the increased effect due to the flow of lines in the space occupied by the magnet considered as if no iron were present.

SEC. 16. The conception is strengthened by what we know of the physical effects shown in magnetizing iron and of the results of external forces upon magnetized iron. Villari discovered, and Prof. J. J. Thomson has elaborated the fact, that weak magnetizing forces cause iron to elongate, and that as the magnetizing forces increase, the elongation is lost and an actual shortening takes place proportioned to the magnetization. The writer considers that the initial lengthening may be well explained by the fact that the molecules, lying in various directions, at first need room to arrange themselves in the same general direction, and that therefore the initial effort produces expansion, whereas afterward, when such molecules are arranged approximately in the same direction, they can readily approach each other in response to the polar attraction between their adjacent ends. So also heating at first increases the intensity of the magnetization and afterwards decreases it, which we explain on the ground that it at first helps separate the molecules so as to facilitate their axial re-arrangement, and afterwards serves to separate the re-arranged molecules and prevent as close approximation as would be the case without the increased temperature. Again, elongation in general reduces, and compression increases magnetization with a given exciting force, which corresponds with the considerations above given. Bidwell's experiments show that nickel does not have the so-called "Villari reversal,"[1] which does not conflict with the above explanation, as we have simply to consider that its molecules are originally arranged in proper direction. On the contrary, cobalt is affected in the opposite way to iron, which may be explained by the fact that the molecules are so differently formed that they approach parallelism better by a movement in a different direction.

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[1] See Ewing on "Magnetic Induction." § § 163–171.

SEC. 17. The forces required to deflect the molecular magnets towards parallelism during the initial rise in the curve must on this theory be proportioned principally to the modulus of elasticity of the iron used. It may be that the angular change in direction of the molecules must also be considered, which will complicate the problem. The other features of the curve can evidently be explained by the extreme forces necessary to deflect still nearer to exact parallelism magnets tended to be held by definite elastic forces out of parallel, and to the energy that can be stored during these reactions must be added the flow through the space occupied by the magnets as if no iron were present. The action on the molecular magnets tends to a limit, and any curve plotted therefrom must therefore be an asymptote, and superimposed thereupon must be the linear values due to the direct action of the exciting force in moving lines through space independent of the special qualities of the iron in such space.

SEC. 18. By carefully reviewing the considerations expressed in Sections 15 to 17 inclusive, it will be observed that in presenting the Weber–Maxwell–Ewing theory of molecular magnets and thestrain diagram, Fig. 2, in connection therewith, as a means of finding a rational reason for the greater initial rise of the H-B curves of magnetization, than is shown by the fluid function, Eq. (10), such theory of molecular magnets offers an interesting and somewhat plausible explanation of the whole phenomena without necessarily involving the theory of etheric flow. The writer, however, contends that *etheric motion of some kind necessarily follows from the phenomena of magnetic action at a distance* (Sec. 2); that magnetic phenomena rather indicate a flow than a transfer of vibrations; that the fluid function plotted in a curve shows a general similarity to an H-B curve and a limiting condition analogous to magnetic saturation (Sec. 9); that such fluid function therefore confirms the theory of translatory rather than vibratory movements of the ether to explain magnetic phenomena, and that, therefore, such function is of the highest importance as evidence to connect magnetism with ethereal flow. On the contrary, the theory of molecular magnets does not aid in the explanation of action at a distance, but is still of value to intensify the effects shown by the fluid function.

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[1] Ewing on "Magnetic Induction." § 143.

SEC. 19. Recurring to the considerations expressed in Sections 15 to 17, we may allow that the elastic reactions due to molecular attraction measure the intensity of the possible magnetization during the initial stages shown on the diagram, Fig. 3, up to the abrupt turn, but it is not believed that the highest possible magnetizing force can equal, much less exceed, the high resistance to expansion or compression shown by the strain diagram, Fig. 2. Granting the theory of molecular magnets and the swinging of the same by magnetizing forces, such swinging, resisted by such great forces as are shown by the strain diagram, must be through very limited arcs. It is believed, therefore, that the molecular magnets simply act to intensify the magnetization through the range it is possible to swing the same, and that higher magnetizations to the point of saturation are in the main governed by the laws of the flow of fluids exemplified by the fluid function developed above. In the "Conclusion," at Sec. 44, it is shown that the intensification due to molecular magnets may be considered in Eqs. (10) and (10a) by simply changing the exponent of the fluid function and a very gratifying accordance with experimental results is there obtained on such basis.

SEC. 20. In closing this branch of the subject, it will be of interest to consider how other physical phenomena can be explained if it be finally settled that magnetism is due to the circulation of an ethereal atmosphere. In such case we must imagine that the ether pervades all space, and we may suppose it susceptible to vibrations and therefore capable of transmitting vibratory forms of force, such as light, without interference with the phenomena or magnetism due to the circulation of its ultimate particles. This atmosphere need not necessarily be the luminiferous ether, though there is no reason why it should not be so. The etheric fluid itself may pass freely through all substances, gaseous, liquid or solid, and yet its vibration known as light be damped and checked by some of such liquids and solids, but flow freely in others known as transparent. It will be more difficult to consider one mode of motion of the same ether as light and another as electricity, for both modes of motion should be damped by the same substances that dampen light, whereas we know that light is readily transmitted through glass and electricity is not, and that electricity is readily transmitted through copper and a similar class of substances known as conductors, which do not transmit light. If magnetism be the circulation and electricity a form of vibration of the same ether, there would be a more direct transformation of electric into magnetic force, whereas we know that no direct transfer takes place. A magnetic flow and an electric current may be moving at the same time in the same piece of metal, an iron magnet for instance, without one modifying the other in any way.

SEC. 21. Any explanation founded on the analogy of the polarization of light should be proved from some direct transformations of electricity into light without heat. Tesla appears to be doing this, but it is believed that his results can be explained on the basis that the passage of a high potential and high frequency alternating current through a low conducting medium actually puts the ether in vibration, thus producing light as above indicated. It is believed also that the apparent transmission of high tension discharges through the dielectric instead of the conductor will eventually be traced to magnetic phenomena, leaving us free to consider substantially as in the past that electrical phenomena are due to a form of motion of the molecules of solids which are susceptible to this kind of motion, like metals of the copper class, though the molecules of all substances, even of gases, and possibly of ether, are conductors in a limited sense or under peculiar circumstances.

SEC. 22. The subject has proved of great interest and has been considered of such importance that it is submitted to the profession in its present stage of progress in order that the minds of others may become occupied in the same direction and aid in the complete solution of the complex problem, using as much of the above as may survive the test of criticism and of further experiment.

PART II.

SEC. 23. We will now proceed to the second branch of the inquiry, viz.: tne development of empirical formulæ to represent typical H-B curves of magnetization. Such curves, though empirical, will enable equations to be completed, involving varying values of H or B, and thereby permit the application of the mathematical laws of maxima and minima with all the important conditions of the problem included. Empirical formulæ serve also to correct errors in particular records by general comparison through the whole range of the observations and at the same time furnish a more ready and accurate means than arithmetical interpolations to obtain intermediate values really dependent upon complex functions.

SEC. 24. Many approximate formulæ have been suggested for the purpose, of which one of the forms proposed by Frolich, of an expression developed by Lamont and others, has been most generally used. Frolich's equation, in the form

$$B = \frac{H}{a + b\,H} \tag{11}$$

to which all the other forms may be reduced, will, with given constants a and b, approximately represent a considerable portion of some H-B curves of magnetization. In Fig. 3 the application of Frolich's formula is shown in dotted lines in connection with an H-B curve shown in full lines for a Norway iron ring, based on experiments of Rowland in 1873 and presented in a recent paper of Mr. Kennelly.[1] The Frolich function contains

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[1] "Magnetic Reluctance," by A. E. Kennelly, TRANSACTIONS, Nov., 1891, vol. viii., p. 492. If Eq. (11) in text be divided through by B, we have

$$\frac{H}{B} = \rho = a + b\,H \tag{12}$$

which is the linear equation of the reciprocal of the permeability, or of the "reluctance" ρ, given by Mr. Kennelly. This shows that the Frolich and Kennelly formulæ are identical, though stated in different terms. The constants developed by each are applicable in the formulæ of either, and, in the only two constants, so that it can only be passed through any two points of a curve, and is therefore frequently unsatisfactory except very near the points of application.

Sec. 25. The writer has found it of advantage in developing empirical formulæ to use well-known curves which bear a general resemblance to the curves to be fitted. *H-B* curves of magnetization bear some resemblance to those given by ordinary logarithmetic formulæ, and also resemble a simple hyperbola, though

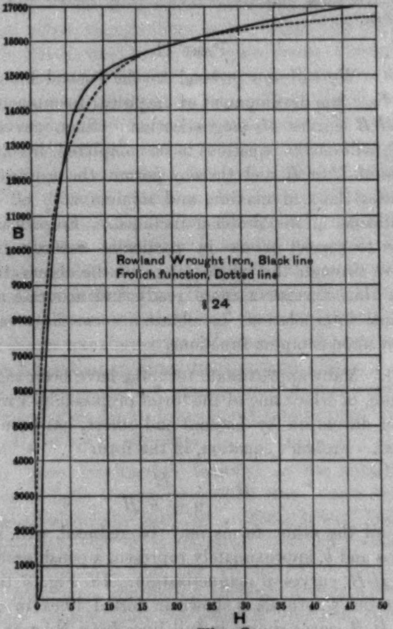

Fig. 3.

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application above, made the value of the reluctance for the second branch of the reluctance curve obtained from the above experiments by Mr. Kennelly is used as the denominator of the fraction in the Frolich formula, Eq. (11). Other applications are given by Frolich in his original papers, in *Electrotechnische Zeitschrift*, 1881, 1882, and by Mr. Kennelly in the paper referred to.

the two branches are rarely symmetrical. An equation well adapted for practical use is that of an hyperbola in the form

$$B = c + \frac{b + H}{a} \tag{15}$$

There being three constants in this equation, the resulting curve can be passed through any three points of any experimental curve by deriving the values of the constants by simple substitution.[1] Some H-B curves of magnetization may be roughly shown by one application of this formula, but by making one application to include the limit from $B = 0$ to some definite point above the abrupt turn, with the third point on or just below the turn, the curve will generally be accurately shown between these limits, and another application, made to three points above the higher limit, will generally show very accurately that branch of the curve. Moreover, the two curves extended will generally coincide practically for a little distance either side of the junction, and it is possible to make the first differential coefficients of the two alike and thereby join the two curves at the same point and at the same angle and make the curve continuous, but this refinement will rarely be found necessary. No attempt is made in this connection to apply the formula to the sharp bend near the origin, marking the excitation necessary to cause the molecular magnets to turn toward approximate parallelism (Sec. 16), but the application can readily commence at excitations of 2 to 3 or 5 for soft iron and steel respectively, and this has a rational basis as shown in Section 41.

SEC. 26. The first application of this curve will be made to the experimental determinations of Mr. Steinmetz, given in one of the series of experiments made in connection with his researches on the law of hysteresis.[1] In these experiments three different samples of sheet iron were tested, one from a Westinghouse converter and the other two from iron of different thicknesses used in the Eickemeyer manufactory, and all showed a remarkable uniformity in result. The experimental values of B given in Table III, p. 15 of the paper of Mr. Steinmetz, are reproduced in

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[1] It would be more rational to separate H and I in these formulæ somewhat as in Eq. (10a), but in this branch of the subject we are only seeking to fit formulæ to expermental curves, which can be done as well with the equation in its simpler form.

[1] Steinmetz on the Law of Hysteresis. TRANSACTIONS, vol. ix, p. 1.

TABLE I.,

Showing, in different terms, the magnetomotive forces and resulting magnetizations of wrought-iron, in connection with experimental results of Steinmetz, Ewing and Hopkinson, and calculated values from empirical and general formulæ.

1	2	3	4	5	6	7	8	9	10	11
	$1.257H_1$	$6.4514H_1$	$3.109H_1$		$6.4514\ B$					
ELECTRIC EXCITING, OR MAGNETOMOTIVE FORCE.				MAGNETIZATION IN MAGNETIC LINES.				Exciting force.	MAGNETIZATION.	
									GENERAL FORMULA (29) and (30)	THOMSON (see Table 2.
Per square centimetre.		Per square inch.		Per sq. centimetre.	Per sq. inch.	Per square centimetre.		Lines per sq. centimetre.		
				STEINMETZ.						
Amp. turns. H_1	Mag. lines. H	Amp. turns. H_2	Mag. lines. H_2	B	B_1	B Form'la (16a)	B Form'la (16b)	H	B	B
1	1.26	6.45	8.11							
2	2.51	12.90	16.22	1700	10967	1700				
3	3.77	19.35	24.33	4200	27096	....				
4	5.03	25.81	32.44	7400	47740	7760	....	5	10086	10000
5	6.29	32.26	40.55	9800	59353					
6	7.54	38.71	48.65	10400	67095	10400				
7	8.80	45.16	56.76	11260	71998					
8	10.06	51.61	64.87	11850	76449					
9	11.31	58.06	72.98	12470	80449	....	....	12	12360	13000
10	12.570	64.51	81.09	13070	84320	12797				
12	15.08	77.42	97.31	13750	88707					
14	17.60	90.32	113.5	14260	91997	14215				
16	20.11	103.2	129.7	14600	94190					
18	22.63	116.1	146.0	14900	96126	14941	14944			
20	25.14	129.0	162.2	15200	98061	15202	15200			
25	31.43	161.3	202.7	15700	101287	15682	15785	28 5	14494	15000
30	37.710	193.5	243.3	16200	104513					
35	44.00	225.8	283.8	16680	107609					
40	50.28	258.1	324.4	17050	109996	....	17184			
45	56.57	290.3	364.9	17500	112900					
50	62 85	322.6	405.5	17900	115480	....	17901	52	16036	16000
55	69.14	354.8	446.0	18300	118060					
60	75.42	387.1	486.5	18650	120319					
65	81.71	419.3	527.1	19030	122770					
70	87.99	451.6	567.6	19380	125028	....	18996			
75	94.28	483.9	608.2	19730	127286					
80	100.6	516.1	648.7	20080	129544					
85	106.9	548.4	689.3	20400	131609	....	....	105	17945	17000
90	113.1	580.6	729.8	20750	133867	....	19796	200	19760	18000
	502.	....	....	....	......	....	....	502	21800	
	1002.	....	....	....	....	....	... ...	1002	23300	
EWING.										
	1490	....	......	22650	....	....	....	1490	22790	
2865	3600	18483	23232	24650	159027	....	24649	3600	24900	
	8600	....	....	30270	....	....	......	8600	29900	
	19450	......	... .	40820	....	....	....	19450	40750	

column 5 of Table 1 of this paper, and the exciting forces given in ampere turns per square centimeter in the paper of Mr. Steinmetz are here reproduced in column 1. The exciting forces in terms stated, herein designated (H_1) are so regularly distributed that the opportunity has been embraced to add for the purposes of comparison the corresponding number of magnetic lines per square centimeter (H); also the corresponding number of ampere turns per square inch (H_3) and the corresponding number of magnetic lines per square inch (H_2). There are also added in connection with the magnetizations in magnetic lines per square centimeter the corresponding magnetizations in magnetic lines per square inch (B_2).

SEC. 27. The equations in the form of Eq. (15) showing the magnetizations B (mag. lines per sq. cm.) in terms of the exciting forces H_1 (amp. turns per sq. cm.) are as follows:

For $H_1 = 2$ to 20 (Steinmetz)

$$B = 17730 - \frac{54021}{H_1 + 1.37} \tag{16a}$$

For $H_1 = 20$ to 2865 (Steinmetz)

$$B = 24908 - \frac{755962}{H_1 + 57.87} \tag{16b}$$

The corresponding equations showing the magnetizations B_2 (mag. lines per sq. inch) in terms of the exciting forces H_3 (amp. turns per sq. inch) are as follows:

For $H_3 = 13$ to 129 (Steinmetz)

$$B_2 = 114383 - \frac{2248380}{H_3 + 8.838} \tag{16c}$$

For $H_3 = 129$ to 18500 (Steinmetz and Ewing)

$$B_2 = 160692 - \frac{31463500}{H_3 + 373.34} \tag{16d}$$

SEC. 28. Several values calculated by formulæ (16a) and (16b) are given in columns 7 and 8, Table I. The largest variation for formulæ (16a) appears for $H_1 = 4$. The Steinmetz results at this point are phenomenally low, as will be seen by comparison with the corresponding value for Hopkinson's experiments in column 11. It has been thought desirable to present these careful experiments by Mr. Steinmetz with the Eickemeyer apparatus for comparison with other experiments, but it will be observed by comparing Tables 1 and 2 that for all values of B, greater than for $H_1 = 10$, the Steinmetz results are higher than those customarily given, so much so in fact as to suggest hesitancy in using the same until the methods of observation are thoroughly discussed. It has been thought that the three specimens of sheet iron tested by Mr. Steinmetz would on analysis prove to be steel, but the results are even higher than those obtained for steel in the Cornell experiments hereinafter referred to, as shown in Fig. 4.

Sec. 29. As a matter of interest the upper limit used in determining equation (16*b*) was based on Ewing's experiments[1] with exceedingly high magnetizations, the value selected, as seen at bottom of Table I, being for $H_1 = 2865$, $B = 24650$. This fact makes the results from Eq. (16b) fall below the higher of the observed Steinmetz results. There would have been no difficulty in confining the formula to the range of observation, when it would have fitted more accurately throughout its whole length as shown in subsequent applications. The interest attached to a formula reaching so nearly to the extreme limits of observation has been thought to warrant this application. It will be observed however that the equation only fits the one value from Ewing selected. The others can only be reached through a more general formula like Eq. (26) to be discussed hereafter. (See column 10).

Sec. 30. The second application of formula (15) has been made to the tabulated results given at page 143 of the 4th edition of Prof. S. P. Thompson's work on "Dynamo Electric Machinery." This table, as is known, was obtained by averaging the ascending and descending values of B obtained during experiments by Hopkinson on wrought-iron. The empirical formula showing approximately the tabulated magnetizations B (mag. lines per sq. cm.) in terms of the exciting forces H (mag. lines per sq. cm.) are as follows:

For $H = 1.66$ to 52 (wrought-iron)

$$B = 16819 - \frac{55656}{H + 3.049} \tag{17a}$$

For $H = 52$ to 666 (wrought-iron)

$$B = 20816 - \frac{848915}{H + 117.5} \tag{17b}$$

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[1] Ewing on "Magnetic Induction." Ed. 1892, Table XII, p. 144.

TABLE II.,

Showing magnetomotive forces and resulting magnetizations of wrought-iron, in different terms, in connection with average results of experiments by Hopkinson, tabulated by Thompson, and calculated values from empirical formulæ; showing also Cornell experimental results with mitis metal and calculated values from empirical formulæ connecting the initial results for mitis metal with the Hopkinson results for wrought-iron.

1	2	3	4	5	6	7	8	9
ELECTRIC EXCITING OR MAGNETOMOTIVE FORCE.			MAGNETIZATION IN MAGNETIC LINES.					
Mag. lines.	Ampere turns.		Per sq. cm.	Per sq. inch.	Per sq. cm.	Per sq. cm.	Per sq. cm.	Per sq. cm.
Per sq. cm.	Per sq. cm.	Per sq inch.	THOMPSON.		FORMULA (17a)	FORMULA (17b)	CORNELL mitis No. 22.	FORMULA (18a) Compromise.
H	H_1	H_2	B	B_2	B	B	B	B
1.66	1.32	8.52	5000	32257	5000			
4.0	3.18	20.53	9000	58063	8924		6250	6617
5.0	3.98	25.66	10000	64514	9904		7750	7729
6.5	5.17	33.36	11000	70965	10991		9100	9038
8.5	6.76	43.62	12000	77417	12000		10450	10335
12.0	9.55	61.58	13000	83868	13121		12000	11875
17.0	13.5	87.24	14000	90320	14043		13350	13230
28.5	22.7	146.3	15000	96771	15055	15001	15000	14817
52.0	41.4	266.9	16000	103222	15808	15808	16300	16072
70.0	55.7	359.2				16289	16850	16502
105.0	83.5	538.9	17000	109674		17001		
200.0	159.1	1026.	18000	116125		18143		
350.0	278.4	1796.	19000	122577		19000		
666.0	529.8	3418.	20000	129028		19733		

For exciting forces H_2 stated in ampere turns per square inch and magnetizations B_2 in magnetic lines per square inch, these equations take the form

For $H_2 = 8.5$ to 267 (wrought-iron)

$$B_2 = 108506 - \frac{1842826}{H_2 + 15.649} \tag{17c}$$

For $H_2 = 267$ to 3420 (wrought-iron)

$$B_2 = 134292 - \frac{28108425}{H_2 + 603.06} \tag{17d}$$

SEC. 31. On account of the tables, etc., only the preliminary paragraph of the second part of this paper was read at the General Meeting, so this part of the paper has been revised to include the consideration of the experiments presented at such meeting by Dr. Nichols recorded in the paper of Messrs. Thompson, Knight and Bacon on "The Magnetic Permeability of Special Irons for Electrical Purposes." As these experiments were part of the regular course of study in the Department of Electricity at Cor- University, it is due to that institution and Dr. Nichols, and prevents confusion, without disparagement of the excellent work of still another Thompson and his associates, to refer to such experiments as the "Cornell" experiments, designating the curves by the numbers written thereon in the original paper. See Fig. 4.

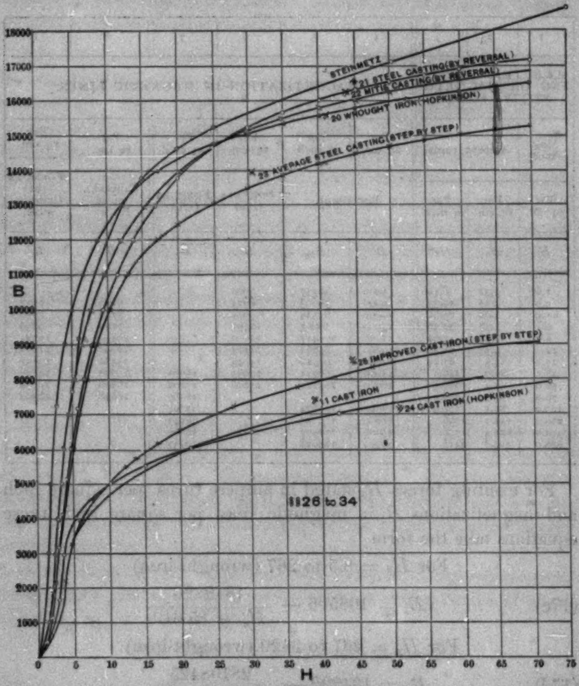

Fig. 4.

SEC. 32. An examination of the Cornell experiments shows clearly what we had previously gathered verbally from Mr. Steinmetz, and what has probably been known to large manufacturers for some time, that some samples of soft steel, either in the form of castings or plates, though showing less magnetization for low exciting forces than iron, really attain a higher magnetization than iron as the exciting forces are increased. Though the Cornell paper shows the latter fact in regard to many specimens, it is not true of all, so it is thought that in practice without tests, higher values than those of S. P. Thompson for wrought-iron should not be considered. The initial magnetizations shown by wrought-iron are, however, too high for steel and mitis metal, so an empirical equation has been developed in the form of Eq. (15) to include both limitations. The initial values have been made to imitate the Cornell results for specimen No. 22 of mitis metal, the results from which are a fair average of those from iron and steel as may be seen by comparing column 8, Table 2. The higher values by equation have been caused to follow closely the Thompson–Hopkinson values, column 4, Table 2. This compromise curve can, between the limits of the Cornell experiments, be expressed by one equation as follows:

For $H = 3$ to 70 (Compromise iron and steel)

$$B = 17868 - \frac{102576}{H + 5.1165} \tag{18a}$$

For $H_2 = 14$ to 350 (Compromise iron and steel)

$$B_2 = 115274 - \frac{3396394}{H_2 + 26.26} \tag{18c}$$

Several values from Eq. (18a) are given in column 9, Table 2.[1]

SEC. 33. The Cornell curve No. 23 showing magnetizations for average steel castings can, by slightly reducing the lower values, also be expressed by one equation, as follows:

For $H = 5$ to 70 (Av. steel casting)

$$B = 17035 - \frac{124740}{H + 6.304} \tag{19a}$$

For $H_2 = 25$ to 360 (Av. steel casting)

$$B_2 = 109900 - \frac{4130266}{H_2 + 32.3546} \tag{19c}$$

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[1] Since writing the above it has been found that the Frolich function, Eq. (11), which, as has been explained will not fit closely some forms of H-B curves, can be adapted to show satisfactorily a compromise steel and iron curve throughout a considerable range. The tendency of the function to decrease the higher values, can be overcome by making another application of the formula through a different range. The advantage is that, when a table of reciprocals is available, the arithmetical solutions are even more simple than for the equations

SEC. 34. The curve, No. 24, Fig. 4, for cast-iron, based on experiments by Hopkinson, given by Thompson at page 302, 3d ed. of his book previously referred to, shows results approximately agreeing with those from ordinary American cast-iron, though an improved quality gives higher results, as shown in Cornell No. 25, Fig. 4. The Thompson–Hopkinson curve may be formulated as follows:

For $H = 5$ to 80

$$B = 9572 - \frac{164201}{H + 24.469} \tag{20a}$$

For $H = 80$ to 127

$$B = 6300 \times 21.3\, H \tag{20b}$$

a simple rectilinear function which will answer, for the limits stated and the rarely used higher results may be obtained from a table.

For $H_3 = 25$ to 400

$$B_2 = 61753 - \frac{5436859}{H_3 + 125.58} \tag{20c}$$

For $H_3 = 400$ to 615

$$B_2 = 40644 \times 26.774\, H_3 \tag{20d}$$

The Cornell experiments show for some specimens of cast-iron higher results than those given by Thompson, but for some of the results a falling off during low magnetizations similar to that for steel is to be observed. When high magnetizations are to be secured in any case, a safer number of field turns will be secured by using a curve of what may be called the "delayed action" or steel form. We accordingly give a single

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above given. For this purpose we preferably apply Eq. (11) by dividing through by H, so as to show the permeability, μ, and in this shape the following formulæ will be found very convenient:

For $H = 6$ to 105,

$$\mu = \frac{10^7}{554.2\, H + 3624}; \tag{18f}$$

For $H = 105$ to 184,

$$\mu = \frac{10^8}{5.182\, H + 80}; \tag{18g}$$

For $H_3 = 30$ to 540,

$$\mu = \frac{10^7}{108\, H_3 + 3624}; \tag{18h}$$

For $H_3 = 540$ to 943,

$$\mu = \frac{10^8}{H_3 + 80}. \tag{18i}$$

Compromise Iron and Steel.

equation for a curve closely resembling Cornell No. 11, which gives lower initial values for cast iron than those of Thompson–Hopkinson, but slightly increases the higher values. Such equation is

For $H = 3$ to 70 (Compromise cast-iron)

$$B = 9980 - \frac{168120}{H + 19.476} \tag{21a}$$

For $H_3 = 14$ to 360 (Compromise cast-iron)

$$B_2 = 64385 - \frac{5566621}{H_3 + 99.9586} \tag{21c}$$

SEC. 35. It is believed that the several examples above given can be advantageously employed in practice, and that the same illustrate the applicability of the system so that others can construct curves specially adapted to the particular material used. In countries where English units are employed the H_3-B_2 formulæ will be found a great convenience, as from them the magnetization in magnetic lines per square inch can be obtained directly from the number of ampere turns per square inch without first ascertaining the several results in lines per square centimeter, and the results can readily be compared with those on another basis by reference to Table 1. The numbers of each H-B equation referring to square centimeters are marked sub-*a* and sub-*b* for the earlier and later portions of the curve respectively, and the same numbers with sub-*c* and sub-*d* added give the corresponding H_3-B_2 results for square inches. For some pur poses it may be convenient to read magnetizations approximately from a curve on a large scale or from a table, but there are many cases when this will not answer, and it will be found that a particular value can be ascertained by making the full calculations from the formula about as quickly as by interpolation and with much greater accuracy and satisfaction. Independent of this the formulæ are of great value for use under the conditions stated in Section 23. When B_2 equations of magnetizations per square inch are used, other formulæ in which the values are substituted may require modifications, but the same are of a simple and evident nature.

CONCLUSION.

SEC. 36. The previous discussion in Parts I and II of the theoretical and practical phrases of the general question now form a sufficient foundation for the development of a general equation.

It will be observed that in the hyperbolic formulæ, Eq. (15), the first term *c* of the second member really represents, so far as this form of equation can, without separating I as in Section 7 and Section 40, the number of lines which will saturate a given material through the range to which the formula is applicable. For instance, the apparent saturation limit for Eq. (17a) is 16819, but we also observe that the apparent saturation limit for Eq. (17b) is 20816, or considerably higher than in the other equation applying to the initial portion of the same curve. These formulæ appear, therefore, to sustain a view which has been expressed by others that the saturation limit rises as the exciting force is increased. This apparent confirmation is, however, due to the inapplicability of the particular formulæ to represent the conditions for widely different values, though it is quite sufficient to merely show between limits the relations of *H* and *B*. It should be noted that the upper branch of the curve being an asympote will continually approach the limiting value *c* in Eq. (15), but never reach it.

SEC. 37. Ewing states distinctly (p. 149), that saturation had been definitely reached within the limits of his experiments, though, as stated in different language, additional lines were added above that limit by the action of the solenoid as if no iron were present. It may readily be shown that his experiments sustain these views. If we subtract the values of *H*, accredited to Ewing in Tables 1 and 3, from the corresponding values of *B*, we have, as shown in the third column of Table 3, headed *B — H*, a practically constant quantity, which may be called absolutely so in view of the difficulty in making close experimental readings under such extraordinary conditions. The aver-

TABLE 3, BASED ON EXPERIMENTS OF EWING.[1]

H	B	$B - H$
1490	22650	21160
3600	24650	21050
0070	27130	21060
8600	30270	21670
18310	38960	20650
10450	40820	21370
19880	41140	21260
		7) 148220
		$K =$ 21174

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[1] See foot note. § 29.

age of the several values of $B - H$ is 21174, and this must certainly mean the maximum or limiting value of I, Section 7, or the number of lines representing the saturation of the particular material experimented upon.

Sec. 38. Table 3 also shows that at these high limits the magnetization increases directly with the exciting force and is

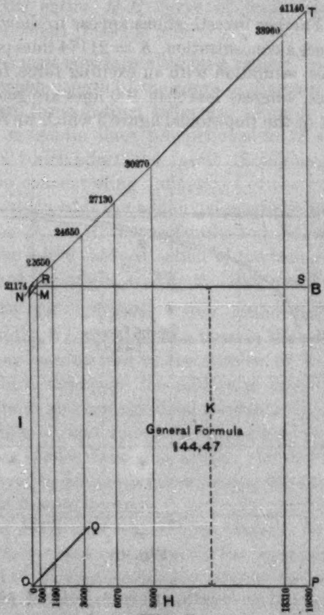
41140
T
38960
30270
27130
24650
22650
R
S
21174
N
M
B
I
K
General Formula
144,47
Q
O
P
0
500
1480
3480
5960
9090
17790
19960
H

Fig. 5.

numerically equal to the saturation value of I, plus the exciting force H, and indicates that this law extends from these high values back to the origin where the exciting force and magnetizations are zero. If in Fig. 5 we let the ordinates of the triangle $R\ T\ S$ represent the lines of magnetization due to the solenoid, or substantially in the language used in Section 17, to the number of lines circulated through the space occupied by the iron as if no iron were present, the base and altitude of this triangle will each equal H, and on the principle above stated, this triangle must be superimposed upon a rectangle of which the base $= H$ and the altitude equals the saturation limit of the iron or I maximum which we will call K. The figure has purposely been drawn to scale to include the highest values given by Ewing (See Table 3), viz.: for $H = 19880$, $B = 41140$.

Sec. 39. Further investigations appear to show that the iron which produces a concentration, $K = 21174$ lines per square centimeter reaches saturation with an exciting force H_s between 500 and 2000 lines, whereas less than 100 lines are generally used, so the only part of the trapezoidal figure 5 which we refer to in practice is about $\frac{1}{200}$ of its length, or about $\frac{1}{5}$ of the portion included between the ordinates at the origin and the parallel line close thereto marked 500. In order to ascertain how the ordinary H-B curve of magnetization applies within this limit, it is necessary to make another view, Fig. 6. extending from $H = 0$ to 500, the range included between the first two ordinates in Fig. 5, but with the horizontal scale magnified. In this view $R\ T_1\ S_1$ represents the initial portions of the triangle $R\ S\ T$ in Fig. 5, and $O\ R\ S_1\ P_1$ represents similarly a portion of the rectangle below the same. M represents a modified curve of magnetization

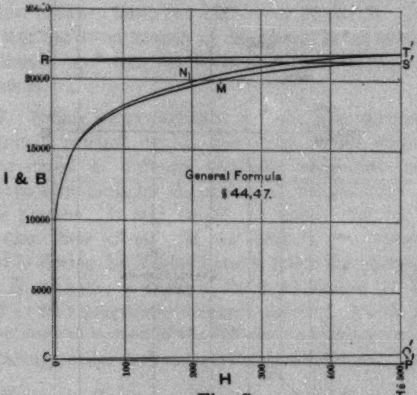

Fig. 6.

in which the ordinates of an ordinary *H-B* curve are shortened by the corresponding values of *H* when measured from the base $O\,P_1$, and *N* represents the same curve of reduced height when the ordinates are measured from an inclined line $O\,Q_1$ parallel to $R\,T_1$.[1] The curve gore between *M* and *N* has the same area as the portion of the triangle from the ordinate at the origin to the point of tangency of the curve, so the upper curve *N* represents the entire *H-B* curve of magnetization if its ordinates be measured from the base $O\,P_1$.

Sec. 40. We may now formulate on a rational basis the values of *B* for the whole length of the curve or diagram above developed. There are three distinct influences acting to produce this curve, 1st, the magnetic lines proportioned to *H*, shown by the triangle *R T S*, which affect the result throughout the entire range; 2nd, the concentrating influence *I* of the iron, the maximum value of which adds the additional magnetic lines *K* shown by the altitude *O R* of the rectangle, and 3d, the influence which causes the initial delayed action of the iron, shown by the reversed curve at the origin, *O*, Fig. 6. (See also Fig. 4.)

Sec. 41. The 1st influence above stated may be simply expressed by adding *H*, variable, as a term in the equation. The 3d influence may be measured by the number of lines necessary to expand the core, disengage the molecular magnets from their accidental positions and arrange them substantially parallel, as explained in Section 16, and as may be represented by starting the curve a definite distance from the origin. Of course a function could be employed to develop a curve joining the origin with the main curve, but it is unnecessary in ordinary practical work. If we represent by *k*, the number of lines required for the purpose above explained, we have but to use in the equation developed, $H - k$ instead of *H*, thus practically causing the curve to intersect the axis of abscissæ at a distance, *k*, from the origin.

Sec. 42. The second influence, *I*, due to the action of the iron in initially building up the magnetization, is, as has been previously discussed, Sec. 7, a function of *H* or $(f\,H)$ acting only from $H - k$ to the point of saturation. If we let H_s equal the number of lines of exciting force required to produce saturation and *K* the magnetization at saturation, due to the iron alone, we may write

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[1] This is the *I-B* curve of Ewing.

From $H = 0$ until $H = H_s$ and $(f\,H) = K$

$$B = (H - k) + (f\,H) \tag{22}$$

When H is greater than H_s

$$B = (H - k) + K \tag{23}$$

Sec. 43. We may base the function of H, $(f\,H)$ in Eq. (22) on the fluid function, viz.: $(3\,r_2 - 2\,r_3)^{1/2}$ developed in Section 7. In this function it will be recollected that r is a ratio varying only from 0 to unity. The value of r in this case is evidently the actual number of lines available for excitation, viz.: $H - k$ divided by the exciting force at saturation, viz: H_s, or

$$r = \frac{H - k}{H_s} \tag{24}$$

Sec. 44. In the form given above the fluid function is, as stated in Section 11, of insufficient intensity. If we consider, as is probable, that the molecular magnets act to change the intensity in accordance with the same laws as the fluid function acts, we can change the intensity shown by the fluid function by simply changing its exponent. As r is fractional, the intensity will be increased by decreasing the exponent. Multiplying such exponent by $\frac{1}{m}$ in which m is, in a sense, the coefficient of intensification, and putting n equal to the product of $\frac{1}{m}$ by $\frac{1}{2}$, the exponent of the fluid function, we have

$$n = \frac{1}{2\,m} \tag{25}$$

Substituting such fluid function with n as the exponent in place of $(f\,H)$ in Eq. (22), we have

For $H = 0$ to $H = H_s$ and $(f\,H) = K$

$$B = (H - k) + K\left(\overline{3\left(\frac{H - k}{H_s}\right)^2 - 2\left(\frac{H - k}{H_s}\right)^3}\right)^n \tag{26}$$

For H greater than H_s, Eq. (23), applies as before.

That is, above the point of saturation the iron ceases to act to concentrate magnetic lines, so the fluid function must be caused to disappear as r reaches its limiting value, unity.

Sec. 45. Before substituting actual values in the above equation we will examine the remarkable properties of the simple equation underlying the fluid function, viz.:

$$y = a\,r^n \tag{27}$$

when r is a ratio less than or not greater than unity and the exponent n is fractional or not greater than unity. If in Eq. (27) $n = 1$ the equation is that of a straight line and the different values of y, as r is varied from 0 to unity, will plot in a diagonal. (See Fig. 1) If $n = 0$ the ordinates for all values of r will equal unity and the same will all plot in the upper horizontal line of Fig. 1. For the lowest conceivable fractional value of n the values of the ordinates in the equation will plot in a right angle. This curious function therefore developes two sides of a triangle as one of its limiting values and the diagonal of the same for its other limiting value, and for values of n, intermediate between 0 and 1, the whole area of the triangle will be filled with similar curves gradually changing in shape from a right angle to the hypotenuse of a right angled triangle. If instead of r a function of r be used, for instance the fluid function shown by the reversed dotted curve in Fig. 1. the limiting values of $(f)\,r$ will be the same, viz.: the right angle and the diagonal, but for decreasing values of n the triangle will be filled with curves modified in shape by that of the reversed curve. We can then intensify the ordinates of the fluid function embodied in Eq. (26). (See Section 43) as described in Section 44, by decreasing its exponent by a multipler $\frac{1}{m}$, so as to develop a curve either departing little from a diagonal as in Fig. 1, or so as to approximate a right angle, or, at will, to take any intermediate position.

SEC. 46. It is evident, in accordance with the principles above explained in relation to Eq. (27). Section 45, that the values of k, K, H_s and n Eq. (26) can be so modified as to embrace the same conditions and trace a curve practically identical with any regular H-B curve of magnetization. The equation is, however, not a simple one to operate, so no attempt has been made to use fractional exponents extended to several decimal places as would be required to fit a particular curve. In a particular application now to be presented, the constant values employed were as follows:

$k = 2.3$, $H_s = 500$, $K = 21300$ and $m = 6.25$ so from Eq. (25)

$$n = \frac{1}{2\,m} = \frac{1}{12.5} = 0.08 \qquad (28)$$

On this basis we have by substitution in Eq. (26)

For $H = 0$ to 502

$$(29) \qquad B=(H-2.3)+21300\left[3\left(\frac{H-2.3}{500}\right)^2-2\left(\frac{H-2.3}{500}\right)^3\right]^{0.08}$$

For H greater than 502 from Eq. (23) we have

$$(30) \qquad B=(H-2.3)+21300=H+21298$$

Sec. 47. A curve derived from the general equation, in the form above is shown at N in Fig. 6, and several values are given in column 10 of Table 1 for comparison with the results obtained by Steinmetz, shown in column 5, and those of Hopkinson by Thompson, shown in column 11. The initial rise shown by this particular form of the general equation is more like that of Thompson than of Steinmetz; the curve then rises less rapidly, and coincides with Thompson for $B = 16000$, after which the general equation values run intermediate between those of Thompson and Steinmetz, and finally correspond with the experimental results of Ewing, shown at the bottom of the table, with a surprising degree of accuracy, considering the difficulties of the problem.

Sec. 48. It thus appears that by the combination of the fluid function with a rectilinear one, a general expression is produced on a rational basis applicable both for moderate magnetizations and the exceedingly high ones given by Ewing. It has been shown in Part I, Section 11, that "the laws relating to the flow of elastic fluids impose no condition antagonistic to the adoption of the theory of etheric flow, but on the other hand the elements of the fluid function vary in the right direction and show a limiting value akin to magnetic saturation." So also, as per Section 18, the fluid function "confirms the theory of translatory rather than vibratory movements of the ether to explain magnetic phenomena." In this conclusion we have shown the adaptability of the fluid function to represent actual experimental results accurately, which furnishes still further evidence to prove the identity of magnetism with an etheric flow, which can be intensified by molecular magnets, thus utilizing in part the theoretical work that has has been done in that direction, as outlined in Sections 13 to 17, and providing at the same time for action at a distance, Section 2 and Section 18, through a medium extending through such distance.

A paper read at the General Meeting of the American Institute of Electrical Engineers, Chicago, Ill., June 6th, 1892. Vice-President Lockwood in the Chair.

A DYNAMO INDICATOR, OR INSTANTANEOUS CURVE-WRITING VOLTMETER.

BY GEORGE S. MOLER.

In connection with the work in dynamo and motor testing in the physical laboratory of Cornell University, it was found desirable that some method should be devised which would enable the student experimenting, to very quickly explore the field of a machine, so that he could study some of the changes which might take place even in a few seconds. The double brush method of Dr. S. P. Thompson, and the single brush method of Mr. W. M. Mordey, are in use and give excellent results, but either of these methods requires a few minutes at the very shortest to obtain the data for a single curve.

To attempt to meet this want the writer devised the instrument which is the subject of this paper. It is intended to be used in tracing those curves which show the changes in potential of the two ends of a single coil of the armature, and also those showing the changes between one brush and one of the commutator bars, as the armature revolves. The instrument consists in the main of a very rapidly vibrating needle of a voltmeter, and a smoked cylinder against which the pointer is made to press at the required moment. The curve is traced during a single revolution of the armature, and is then easily transferred to paper, and is thus placed in a permanent form to be studied at leisure. The voltmeter, which is inclosed in the body of the instrument, is of the permanent magnet form. The needle is of soft iron, and is very short, and held at zero by being placed between the poles of a powerful steel magnet. The pointer is made of aluminium and is quite short. It is shaped so as to be as light as possible, and so that the air cannot offer much resistance to its motion. The cylinder is of metal and very smoothly finished, so that it will offer little resistance to the movement of the pointer. It is mounted upon a short shaft and is arranged so that it can be slid to several different positions; a spring dropping into notches holds it at each place. The stationary pivot at *A*. Fig. 1, is made with a spring so that the cylinder can be quickly removed for smoking or for transferring the curves. This short shaft is connected with the driving shaft of the instrument by projecting pins at *B*, so that it will always maintain the same relative position with the pin at *C*. The pin *C* is attached to a sliding sleeve which is pushed toward the point of the shaft by a spring. A hole or a pin in the end of the armature shaft is to engage with the pin *C*, so that the cylinder will always have the same relative position with the armature. To operate the instrument, two insulated wire hoops or bands are to be wound around some convenient part of the shaft or commutator, and one is to be connected to one commutator bar and the other to one of the adjacent ones. Small brushes are to be made to press upon these bands, and they are to be connected to the binding screws at *D*. The cylinder is then lightly smoked by removing it and revolving it over a candle or gas flame; then it is placed in position. The sleeve carrying the pin *C* is pulled back and latched in the notch *E*. Then the point is pressed into the centre hole in the end of the armature shaft; this will put the drum in motion; then by letting the projecting knob strike the finger the sleeve will be unlatched, and will spring forward and engage with the pin in the end of the armature shaft. Then pressing the button *F*

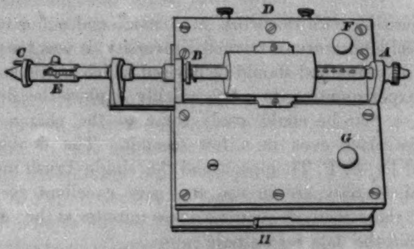

Fig. 1.

closes the circuit, and pressing *G* brings the pointer against the drum. *H* is a reversing switch. The base line of the curve is drawn by pressing *G* only. Another form of curve will be obtained if one of the terminals of the indicator is attached to one of the permanent brushes of the dynamo, and the other to one of the brushes pressing upon the band connecting with one of the commutator strips. Also a temporary exploring coil may be wound upon the armature, and its terminals may be connected to the indicator.

The voltmeter of the indicator is necessarily one of low potential, so that the changes in a single coil will produce a suitable deflection. For greater changes of potential a resistance must be used in series with it. The curves are transferred from the smoked cylinder by dampening a sheet or paper and then rolling the cylinder over it. Three distinct copies have in this way been made from one set of curves. One of these instruments has been built and has been tested in various ways and is found to give very satisfactory results. The rate of vibration of the needle was determined while the pointer was in contact with the revolving drum and was obtained as follows:—A current was applied so as to produce a deflection, then while the drum was revolving brought in contact with it and the circuit was then broken, making a wavy line extending part way around the drum before it died out. The average rate of oscillation as determined from over 40 measurements was 103 per second, and the damping effect was so great that in .04 of a second the needle was brought

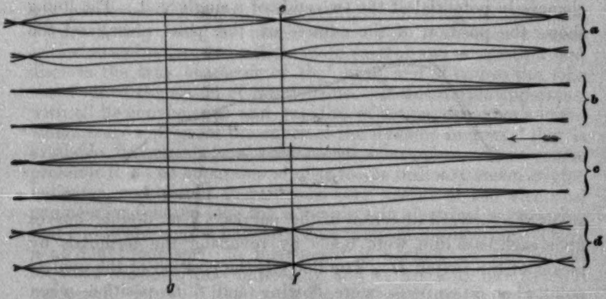

Fig. 2.

to rest. It was also observed that the deflection was not changed by bringing the pointer in contact with the revolving drum.

One of the desirable features of the indicator is the rapidity with which the curves can be made. The cylinder-full of curves, eighteen in all, with their base lines, was made in 73 seconds, so it is seen we can explore the field many times while a dynamo is charging up just after being started, or while any great change in load is being made.

In Fig. 2 are shown some of the curves produced by exploring the field of a 20-ampere shunt-wound dynamo of the consequent pole type. The dynamo was driven at a speed of 1075 revolutions per minute, and had an automatic regulator, which held it at 115 volts, with slight variations. The curves *a* and *b* were made while 20 amperes were flowing in the main line, and *c* and *d* while the outside circuit was broken. The curves *a* and *d* show the changes in potential of the two ends of a single coil. The line *g* shows the position of the centre of the pole piece, and the arrow the forward direction of the curves. The double curves upon each base line, were made by reversing the terminals by means of the switch *H*. The line *e* is the position of the neutral point when 20 amperes were flowing, and *f* its position when the line is open. The difference in shape of the curves shown at *a* and those at *d* is very marked. The curves *b* and *c* were made by connecting one terminal to one of the dynamo brushes, and the other to one of the brushes connected to one end of a coil. These also show the change of the neutral point, and a change in shape for open and closed circuit.

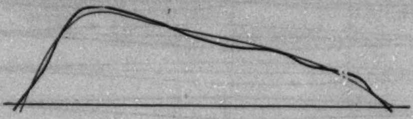

Fig. 3

The instrument is calibrated by comparison with a standard voltmeter. For each reading on the voltmeter a line is drawn around the drum. When the pointer is not in contact with the drum some of the effects of inertia can be noticed, but when contact is made they seem to almost disappear, and corrections may be applied to compensate for the lag on that account.

To test the reliability of the instrument one of the 20-ampere curves for half a revolution and similar to *a* Fig. 2, was taken at random, and compared with data obtained by two students. The heavy line, Fig. 3, gives the result which they obtained, working for at least an hour by the usual method. The fluctuations indicate the extent to which irregularities of speed and voltage interfered with the result. The light line shows the same curve as obtained by means of the "indicator."

Physical Laboratory of Cornell University,
May, 1892.

Discussion.

Prof Crocker:—Mr. Chairman, one point that strikes me in connection with this matter is the fact that the ordinates are so small that the character of the curve is not clearly shown. In other words, the curve is too nearly a straight line to clearly show any particular character. For example, referring to diagrams, Figure 2, they look too much alike. But I suppose a more critical examination, and a little more careful measurement would disclose the true character of the curve, and of course the true character of the action of the dynamo. If this simple apparatus, with all its convenience and rapidity of action, can give a curve which truly represents the curve of the dynamo in a way that is available, it certainly is a very useful instrument. It of course pretends to be an apparatus similar to the ordinary steam engine indicator, and would therefore be very useful to the electrical engineer, and would take the same place in electrical engineering that the steam engine indicator does in steam engineering. There is no doubt that such an instrument is needed—in fact almost required—in electrical engineering, and we would all like to see it perfected. I would like to ask Dr. Nichols if greater amplitude of vibration or height of ordinate could be obtained without introducing errors due to inertia.

Dr. Nichols:—Prof. Crocker's point is very well taken, and illustrates what may be considered the chief fault of the instrument in its present form. It should be said, however, that while these curves are of such small ordinates that they cannot be readily studied directly, they will bear magnifying. The line is a very fine one, and by measuring the ordinates and multiplying them by some convenient factor, a curve can be obtained which can be subjected to close inspection. This is an inconvenience, but I do not think that it vitiates the instrument where the measurements are of enough importance to admit of this method being used. You expend the time after your test has been made in working it up properly, and you get your measurement in a time so short as to be sure that the results are accurate. I have thought a good deal about the question of increasing the amplitude. I do not know whether it would be practical to multiply it many times or not. I think that without great change in the construction of the instrument the maximum is about reached. Still very likely a further development along these lines might lead to an instrument which would be rapid enough for the purpose, and which would give an ordinate that could be readily studied by the eye.

PROF. ROBERTS:—It seems to me, Mr Chairman, that the curves might be enlarged, and at the same time the ordinates increased more than the abscissæ by photographing the curves at an angle.

DR. EMERY:—Mr. Chairman, I would suggest also that the diagram can be photographed through a cylindrical lens, as in that way the ordinates can be lengthened without changing the abscissæ. I will say that a similar experience has been gone through with in connection with the steam engine indicator. It is no trouble to enlarge a diagram if the small original is perfect in itself. Generally it will answer to have the measurements made through powerful magnifying glasses, or by a short-sighted person without glasses, and the method of photographing through a cylindrical lens need only be resorted to when it is desired to enlarge the diagram in one direction for the purpose of publication.

The meeting adjourned until 8 o'clock P. M., at the rooms of the Chicago Electric Club.

Inaugural address of the President at the General Meeting of the American Institute of Electrical Engineers, Chicago, Ill., June 6th, 1892.

COMING DEVELOPMENT OF ELECTRIC RAILWAYS.

BY FRANK J. SPRAGUE.

It is a trite but mistaken saying that electricity is in its infancy. It dropped its swaddling clothes when Morse sent the first telegraphic message. It put aside dresses and pinafores when the dynamo machine and arc light were invented. The incandescent lamp, the telephone, the art of welding, the transformer, are incidents of buoyant youth. The modern electric motor and electric railway mark a vigorous manhood. The truly marvelous development of electric applications of every kind, the accomplishment of many things which in ignorance of the very art, or lack of knowledge of what are now well known facts, and more particularly the great commercial development of the transmission of power, whether for stationary purposes or for electric railways, has lead to many a foolish prediction and idle boast.

This is no age of inspiration, nor time for hopes never to attain fruition. It is above all things a practical age, perhaps too practical, but nevertheless one in which commercial enterprises to be successful must promise either a new field of development or economies in older fields. As the orthodox few have been waiting in sublime faith for many centuries, and will wait for many more, for the fulfillment of ancient prophecies, so too will impracticable electric enthusiasts vainly wait for the millenium when investments are boundless, performances limitless, and efficiencies unity.

It would perhaps have been proper in making my inaugural address to so representative a body as that of the electrical engineers, that I should touch upon the special discoveries and experiments which have recently attracted attention, but there have been so many enthusiastic and brilliant workers that neither the time at my disposal nor the knowledge I possess would permit me to do justice to their work; hence it seems better to take up a subject with which I have been more particularly identified, which to-day commands so much attention, and concerning which there are such conflicting opinions. While finding encouragement in the achievements of our profession, I think the time opportune for a word of caution.

Electric street railways are no longer experimental, nor is their success problematical. Their history for five years is that of an almost unequalled development. Almost within a decade has occurred the first working of a practical electrical railway. In a third of that period there have been put in operation or under contract more than 450 roads, equipped with nearly 6,000 cars and over 10,000 motors, and with over 3,000 miles of track. There is made a daily mileage of not less than 700,000 miles, and over a billion of passengers are carried annually. At least $75,000,000 have been invested in this industry alone; 30,000 horses in a single year have been relieved from the slavery of street car propulsion; stables are disappearing and streets becoming cleaner; luxurious cars are running on smooth, well-built and rigid roadbeds. Dividends have been increased, expenses reduced, investments enlarged, the unproductive has become productive, the impossible possible. Land values have been increased, habitable limits extended, homes created and time saved.

We no longer hear seriously of the dangers of the trolley wire, the failure of service. The time has come when legitimate investment is amply warranted. Electric street railway construction has become a matter of engineering, not experiment. Not only have the smaller towns adopted what is the only available means of current supply, but the larger cities are following their example. St. Louis and Baltimore, Minneapolis and St. Paul, Buffalo and Rochester, Boston and Brooklyn have fallen into line, and latterly even Philadelphia seeks an improved street service, and in New York public interest is being aroused. In the latter city it is, of course, impossible to tell how successful will be the attempt to introduce electric propulsion. There is a strong and in many respects a legitimate objection to overhead wires. Many unsightly poles and badly strung wires have disappeared, and their place taken by an underground service. The general feeling of opposition to poles and wires ought not, however, to act as a barrier to such reasonable and proper introduction of an overhead system of supply as the conditions now existing in that city very properly warrant. I have frequently pointed out the fact that the greatest good can come to the greatest number, especially in the overburdened condition of transit which there exists, if certain of the lines were electrically equipped; wherever, in fact, there would be no street obstruction.

In broad streets, where the tracks occupy only a small portion of the street and are near together, a line of poles of ornate design with arms projecting on either side, can follow the centre of the street, the same poles being used for lighting. Such a street, it seems to me, is Eighth avenue. Then, too, where there is a middle division or park, such as exists in many boulevards, and which is now used for telegraph poles, there is opportunity for an electric construction which would be entirely unobjectionable. On streets occupied with elevated structures, those structures themselves would be used for a practically rigid overhead system.

Among the numerous places in New York where an overhead system could be put in perfect operation, are Central Park west, the Boulevard from Fifty-ninth street up, a part of First and Second avenue lines, the Third, Sixth and Ninth avenue lines, and all the suburban extensions from the annexed district. I am not sufficiently familiar with the streets of Chicago, which is the last remaining city which must consider electric street railways, to suggest a plan. In these large cities, however, one condition should be insisted upon, and if this condition is met in the proper spirit, then much of the objection which has been raised against an overhead system must necessarily disappear.

The construction must be of the very best. The only overhead line allowed should be a contact wire with sufficient strength; main conductors and the feeders should be put underground in proper conduits. There would then be overhead only a wire necessary for the smallest duty and of the requisite strength. In many streets of course the cable will hold its own until an electric conduit or surface contact system shall be proven satisfactory. Impressed by the great development of this great industry and brought face to face with the changes it has wrought, the query is continually made—will the electric motor replace the steam locomotive? It is similar to the older question—will the telephone replace the telegraph? Will the electric light annihilate the gas system—and in all soberness a like answer can be made: — It will not, but it will, as the electric light and as the telephone has done, create a field of its own, and will replace a portion of the service now done by steam. It seems to me that the growth of electric railways will proceed something in this order:—First, the street systems in the various towns, then connecting lines between adjacent towns following the line of highways, then longer connecting lines, either on the track of existing steam lines, or growing bolder, on exclusive rights of way on the same order. Then will come suburban traffic on a larger scale, and freight transfer systems, and finally the more ambitious project of trunk line service under limiting conditions, such as I will specify. It has been very properly said that a man will make the first long ride on electric railways by transferring from one town system to another through connecting links, rather than on individual roads.

This is precisely the process by which great steam systems have been built up, although, of course, starting on a larger scale, and it is but natural that this shall be one step in the development of electric railways. But evidently this natural process of evolution does not offer scope enough for the more enthusiastic, and we are now and again treated to an ideal electric road to be built on plans boldly defying both geography and the abodes of civilization. An air line route according to rules of surveying allowed only in Russia and on the desert of Sahara; abolition of the grades and street crossings; rigid and continuous rails; loaded cars of light weight, each operated by its own motor and making few or no stops; unlimited potentials and undiscovered resistance to insulation; new physiological and engineering laws; indestructible machinery; unheard of powers of braking and new methods of train operation and signaling; around all a clear atmosphere, above all a perpetually smiling heaven, and behind all an unlimited bank account and the unlimited confidence of the investor. These are some of the characteristics of such a road, but perhaps it is only fair to ask, given some of these conditions, what would be the capacity of steam traction?

No one will question the capacity of a motor to do the necessary work required, or to make a speed superior to that of the steam locomotive, provided sufficient energy be delivered to its terminals, but we must deal with existing or probable methods of supply. It is true that the speed at which a train is propelled by steam has only increased about 50 per cent. in sixty years, for in 1832 the "Matthew Baldwin" often made a speed of a mile a minute; but we must not confound speed with power, for while the maximum speed has not been so materially increased, the endurance, the perfection of the mechanism and the economy of performance have made great strides, and the increased speed, which is by no means the maximum possible of a locomotive *per se*, has been attained at much higher powers, and the schedule time has been shortened principally by cutting down grades, straightening curves, filling up ravines, abolishing trestle works, replacing wooden bridges by permanent ones of iron or stone, by the use of heavier and stiffer rails, better switches, improved methods of automatic signaling, the interlocking switch and signal system, and the abolition of grade crossings; in short, by improvements in details and management which permit of higher speeds on more extended sections of road because of greater safety, lower traction coefficients and a greater degree of confidence possessed by the engineer.

All these things are necessary for high speed electric railways, and any general improvement that will be of benefit to the latter must necessarily be of service to the former. Now, any predictions which are made concerning the future of electric propulsion, either in ignorance or disregard of the possibilities of steam duty, and the limitations necessarily existing in all systems of transportation, deserve and will receive little consideration. Hence let us note a few of recent locomotive performances.

Almost every one is familiar with the remarkable run recently made by a Schenectady locomotive hauling a special train on the New York Central Railroad, when the distance of 439½ miles from New York to Buffalo, was made at an average speed of nearly 60 miles per hour, and which was the precursor of the Empire State express, which makes the regular run at an average speed of over 52 miles per hour.

More recently we have accounts of an interesting record made by a well-known writer on two runs between New York and Albany, on which a large number of indicator cards were taken. The weight of the train was about 270 tons. The steam pressure varied from 160 to 170 pounds. From an inspection of about a dozen cards, the indicated horse power varied from 551 horse power at 44 miles to 1,120 horse power at 78.9 miles. At 60 miles per hour, the train resistance is stated to have been 15 pounds per ton, and at 70 miles, 17.10 pounds per ton. About seven pounds of water were evaporated per pound of coal. A remarkable statement concerning this performance was made by Mr. Sinclair, which, while almost incredible, will, if borne out by an analysis of facts, prove to be something of a surprise to those who make their prophecies of the electric economies by comparative statements. In the description of these tests it is stated that the whole trip shows an indicated horse power per hour for an average expenditure of only about 3⅛ pounds of coal per hour. This is far better than many stationary engines.

On the Central Railroad of New Jersey, one schedule rate is 86¼ miles in 89 minutes, which is made where there are a number of necessary slackenings. On May 13th, the time was taken of the speed of a Baldwin compound locomotive for a considerable period of time on one of the regular runs. Ten continuous miles were made in 452½ seconds and five were made in 222 seconds. The fastest time taken was 44 seconds, and the slowest noted was 47. On February 26th a similar compound passenger locomotive running on the same road broke all steam records by running a mile in 39¼ seconds, or at the rate of nearly 92 miles per hour. At this speed the indicator cards showed 930 H. P., and the drivers, which are 78 inches in diameter, were making 395 revolutions per minute. In the very near future, I expect to have the pleasure of riding on a locomotive when going at these high speeds, and I presume my respect for steam locomotion will not be diminished thereby, nor, on the other hand, will my confidence in the possibilities of electric propulsion under the proper conditions. Experimental runs have been made with an electric locomotive at the rate of one mile in 30 seconds, that is, 120 miles an hour, and I confidently expect some day to go at that rate. But it will be under special conditions and not on the regular trunk lines of this country, and it is the height of folly to suggest that these steam trunk lines are to be abolished. In making these very high speed runs, there is not much attempt at maximum economy of coal consumption, the necessity being to generate steam as fast as required by the cylinder, but on taking an average of five trips, I find that there was evaporated 7.19 pounds of water per pound of coal used, and 9.41 pounds of water evaporated per pound of coal consumed. The total weight of train varied from 213 to 241 tons. The personal equations of engineer and fireman necessarily enter seriously into steam operations, and this, compounded of course with the peculiarties of each engine, and the conditions of service, is shown in railroad reports. In this connection, I recently inspected a number of engine sheets. On one, which gave the duty of 25 engines, the average total cost per engine mile was 10.85 cents, of which 2.66 was for fuel. The total cost varied from 6.8 to 19.24 cents, and the fuel (wood) from 1.96 to 4.77 cents. On another sheet, giving the performance of 22 engines, the total cost per engine mile was 14.70 cents, of which the fuel cost 4.61. The total cost varied from 8.82 to 27.98 cents, and the fuel cost from 2.04 to 7.48. In still another, that of the performance of 18 engines, the total cost per engine mile was 14.73 cents, of which the fuel (coal) cost 6.62 cents. The total cost varied from 10.04 to 22.52 cents, and the fuel cost from 3 82 to 13.84 cents.

In discussing the electric system there is often a confusion of statements with reference to economy. Despite the undoubted fact that the electric motor can probably be run at variable high speeds with less variation of economy than can the steam locomotive, we must not forget that in the latter we are considering the economy of the unit as a whole, not merely of the steam cylinders but also of the boiler and the furnace. In electric propulsion a similar comparison of economies must take into account the variable duty of the central station and the losses on the line as well as those in the motor, and where single units are used, the variation in economy of the whole system would be much greater than in the steam locomotive. There will be only a reasonable fixed efficiency of the central station and the line, when the number of units is large enough to make the load on the central stations nearly constant.

Let us now consider another class of duty. Some time ago I made a very careful analysis of the work done on the elevated roads in New York City with a view of determining the coal consumption, and the duty performed by the locomotives. At the time this investigation was made, now nearly seven years ago, there were in use on the Third avenue division 63 trains at one time, running at very close intervals. The weight of the trains was from 80 to 90 tons; the speed was often as high as 20 to 25 miles an hour; stops were made every third of a mile; in short, the duty demanded of the engines was exceedingly severe. The maximum indicated H. P. of the locomotive was found to average about 163 H. P., although on occasions these locomotives have been worked up 185 H. P. Work was divided approximately as follows:

Acceleration in starting 59 per cent., lifting 24.3 per cent., and traction 16.7 per cent. The average H. P. exerted was 70.3 H. P., considerably less than one-half of the maximum. The work on the line was so distributed that there was an almost constant total duty of about 4,500 H. P. The locomotives were on duty 20 hours, but used steam only six hours, and including all losses when standing still and the amount of steam used in braking there was a H. P. development for about 6.2 pounds of coal. I believe that these figures are entirely reliable, and they show a remarkable performance when we consider the class of duty.

An analysis of the coal expenditures showed that, with an efficiency of 60 per cent. and without any of the energy of the train being returned to the line, the relative coal expenditures between steam and electricity would be about in a ratio of 2 to 1, but if the energy of the train was returned to the line to the extent which I believe is possible, then the relative expenditure of fuel would be in the proportion of 7 to 2. Since the coal charge on the four divisions was at that time about $550,000, it can easily be seen that, independent of any question of saving in the care of the structure, and any deduction of depreciation of the motor equipment, the fuel saving would be sufficient to pay a good interest on the cost of electric equipment and a large interest on the cost of electric equipment minus the value of the present engines. I have no reason to doubt the soundness of the conclusions I then made.

You will have here in Chicago, however, a somewhat more advanced condition of affairs. A compound type of locomotive has been adopted for the elevated road service, and I believe it will show an increased economy over that of the operation of the New York roads. Consequently, in discussing the question of economy, it is necessary to get full information concerning the duty which will be done here. In discussing high speed possibilities and limitations, the testimony of Dr. Dudley as given in a discussion of a paper before this Institute, Feb. 24th, 1891, is interesting.[1] There are, generally speaking, three distinct elements constituting the resistance of train movement on a level, and they have a most important bearing when we consider the operation of long or short trains, and at high speeds. One of these elements is the rolling friction of the train in its bearings; with good rolling stock this is about eight pounds per ton. For all reasonable speeds it is probably fairly constant, provided the lubrication is good. Another element is that of air resistance which varies with the shape of the forward end of the train, the condition of the air, the direction of the wind, and the velocity of movement. The third I may call the train lifting or rail bending effort, which depends upon the weight and swiftness of the rails and solidity of the road-bed.

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[1] TRANSACTIONS, Vol. viii, pp. 82 and 86.

Dr. Dudley stated that on the New York Central system he found the trains of about 250 tons when running at a speed of a mile a minute, have a resistance of from 10 to 12 pounds per ton, but that on short trains of two or three cars the resistance sometimes ran as high as 35 or 40 pounds per ton. This is probably due not to any change in the friction of the bearings, but to the fact that the air resistance enters as a much higher exponent of the total. It at once emphasizes the fact that the operation of short trains at high speeds must, no matter how good the track or how favorable all other circumstances, be with a train resistance higher than required by long and well vestibuled trains. Such a shape can be given to the front of an electric locomotive as will make the air resistance not over one-half that presented by a plain surface of equal cross-section and perpendicular to the line of motion, but even this fact does not alter the other, that the resistance per ton must be higher for small trains than for large ones.

Dr. Dudley further stated, in speaking of the influence of stiff rails, that the difference in power required on the "Chicago Limited" when running on an 80 and a 65 pound rail was from 75 to 100 H. P. per mile, that is, somewhere between 10 and 12 per cent. of the power actually developed, and he estimates that with a 105 pound rail, which is nearly twice as stiff as the 80 pound rail, there would probably be saved another hundred H. P. per mile, making a total saving of a quarter by less than doubling the weight of the rail. In his opinion it is perfectly safe to run a steam engine 120 miles an hour on this heavy rail. Such rail improvements increase speed possibilities with present engines, but we have not related the limit of steam duty. Almost all the locomotive work of the United States has been done up to the present with simple engines. Their weight and capacity has been increased, their steam pressure raised until the standard is now about 140 pounds. Within recent years, however, the compound locomotive has come into use, and there is a comparatively large number of them in daily service. The steam pressure has gone up to 180 pounds as a standard, working sometimes as high as 200 pounds, but these are by no means the limits of steam pressure.

On the Paris, Lyons and Mediterranean railway the standard for steam pressure for compound locomotives is 250 pounds. The compound locomotive has still its battle to fight, but I think it would be a rash man who will say that the days of still higher steam pressure are not to come, and that the triple expansion locomotive will never exist. Speed, capacity and coal economy are, however, not the only questions to be considered in railroad operation, and in discussing the general subject, it will be found that the signaling and braking questions at high speeds are serious ones. Undoubtedly an electric train with distributed motors, making the weight of the train available for traction, could by using the motors as dynamos to return the energy of the train to the line from the highest to mean speeds, and then on a local circuit, be more quickly and effectually slowed down and stopped than where shoe brakes are used, and both methods of course would be available. But if using a motor ahead of a train then there will be comparatively little difference in the stopping power. When riding at 60 or 70 miles an hour, it is a very quick stop to bring a train at rest in less that 2,000 feet. This is often as far as any signal can be made out, especially when the weather is at all thick. Hence we may expect to find on electric railroads, if high speeds are to be attained, and quite possibly also on steam railroads, an extension of automatic signaling so that trains indicate on more than two sets of signals. At present the practice is to divide the line into sections, and when a train passes a certain point it sets danger and cautionary signals, dropping the danger signal on the section just preceding, and the cautionary signal on the one behind.

Turning now to the greater powers, we must not confuse the terms "large powered" and "trunk line" work.

These are two statements which I think will need no corroboration. If we had a continuous train movement completely occupying a track system, there can be no question but that its operation from a central source by electricity would be more economical than if operated by steam locomotives. So, too, if a large number of units in reasonable proximity are moved, and the stopping and starting so regulated that the total demand upon the central stations is fairly continuous and equal, then there is no question as to economy of electric propulsion as compared with steam. On the other hand the operation of a single or very few units over a long distance would be so uneconomical and afford so small a return on the investment required, as to make it prohibitory. Between these two, lies the condition of operation where steam and electricity meet on planes of equality; as the number of trains decreases, steam operation is the more economical; as the number increases, electricity must be preferred.

In discussing the use of electricity instead of steam, a well-known steam engineer recently stated that in his judgment it must be conceded that electromotive force for the propulsion of cars will not be economical except for suburban traffic, and upon certain sections of important trunk lines, like the New York Central between New York and Albany, the Pennsylvania system between New York and Philadelphia, and other lines of like character where it is necessary to dispatch a large number of comparatively light trains every day, and at short intervals. The principal field for a power of this kind would be in suburban service long enough to make the ordinary street electric cars unpopular because of the time required, and in such cases as these mentioned above; also for moving freight trains in cities; that is, for the performance of transfer service. This is precisely in line with the argument which I have advanced from time to time, and which I illustrated in a paper before the National Electric Light Association at its convention in Kansas City, where I outlined the possible service between New York and Philadelphia, which I believed to be practicable, and to which I will again refer.

I must repeat that it narrows itself down to the one question as to the number of trains operated between the two terminal points. Make that number of trains sufficiently large, and the electric motor is the best means of propulsion, whether for high or low speed, whether for large or for small cars. Decrease this number, and we must rely on steam propulsion, or, to put it in another way, the answer to the query, "Will electricity take the place of steam for railroad purposes," is:—Only in part, and then only when the number of units operated between the terminal points is so large that the fuel economy would pay a reasonable interest and depreciation of the necessary cost of the central station and the system of conductors.

Of course I do not in this general reply consider those special cases where advantages are to be gained for which there is a return for capital in another direction. Such a case is that of the Baltimore tunnel, where the investment and cost of operation will be greater than that for steam propulsion; in fact, there will practically be no economy in power, because the steam locomotives are not done away with, but simply unused for a period of a little over a mile. There is in this specific case, however, the incidental advantage of doing away with the necessity of a ventilating plant, and yet getting rid of the annoyances incidental to tunnel service.

Trunk line transportation being a great problem, we should not attempt the simultaneous solutions of all the questions involved, but instead, determine what those questions are, consider their sequence and the probability of success, and solve them in their order.

Every system has its limitation. The electric is not exempt from this law, and hence it will set forth what are well-known limiting laws concerning the transmissions of energy. They have been stated time and again, but somehow or another, people often lose sight of them in discussing the questions of investments in large electric railways, so that I think it would be well perhaps to restate and emphasize it.

The weight of copper necessary to transmit a given amount of power with a fixed loss will vary inversely as the square of the electromotive force used.

The distance to which it can be transmitted with a given weight of conductor will vary directly as the pressure.

The distance to which it can be transmitted over a conductor with a given cross-section will vary directly as the pressure.

The weight of copper necessary where the supply station is in the centre of a system is only one-quarter that required if the station is at one end.

The weight of copper will vary inversely as the square of the number of supplying stations properly placed.

The electromotive force required will vary inversely as the number of stations.

Lack of knowledge of these simplex and unalterable laws has led to much misconception of electrical possibilities, and these have not been confined to electrical engineers.

In many of the suggestions which have been made even by practical steam engineers, there has been an unnecessary confusion of the practicable and impracticable, and the specific object which should have been borne in mind has been lost sight of.

Committees have drawn impossible specifications for trunk line service, and demanded of electric motors a capacity and performance superior to that of the best compound steam locomotives. I unhesitatingly pronounce any attempt to build some such machines for the present, certainly unnecessary and impracticable.

The service thus suggested, if at all needed, will for many years be the better performed by the steam locomotive than the electric.

Leaving out of present consideration trunk line work, there are three problems requiring solution in the application of electricity to propulsion on a large scale under conditions existing, for example, in Chicago or in any other place where there is a movement of a large number of trains on more or less complicated tracks, as will be found at almost all terminal railway stations. They are:

First:—The development of an electric locomotive of ample power which shall be as readily controlled as the steam locomotive, and shall be reliable in operation, and shall show a high economy. Of course such a machine must have all the adjuncts which are necessary for train movement.

Second:—A system of conductors and method of supporting the same which can be relied upon for ample supply of current and absolute certainty of continuous contact at all speeds on curves, switches, cross-overs and the multitudinous combination which exists on yard tracks.

Third:—A system of automatic block signaling, which while effective for steam traffic will not be thrown out of operation by the use of tracks as conductors of electricity for a general supply. This is a more serious question than is at first considered, for this use will materially interfere with if not absolutely destroy the utility of what is known as the rail circuit system.

This third problem is one which must necessarily follow the development of the other two, as the automatic signaling systems now existing have followed the development of steam practice.

While I am not by any means thoroughly familiar with the various methods of automatic signaling, I believe I am justified in saying that there is none at present existing, which will meet all the requirements of railway practice, and which can be operated on tracks used by both steam and electric locomotives, where the rail is used as a part of the supply circuit. Some of the best known systems of signaling would be rendered entirely inoperative.

As difficult as it may seem to devise such a system, I believe that it certainly can be developed, but only properly so on a section of track which is more or less experimental on which at the time of operation automatic signaling is unnecessary, but which is actually operated by both classes of service. Such an experimental section would be a combination of single and double tracks with all the varieties of curves, crossings, switches and ladders, such as would be found in any large yard.

It will probably be found necessary to erect a variety of conductors. From the most careful consideration which I have been able to give to the subject, I believe there is one way, and only one way, in which the current can properly be supplied in any complicated system, and that is from the overhead conductor, practically rigid in character, following very nearly the centre line of all tracks and switches with no movable overhead parts, and with return through the rail. The locomotive would then practically be moving between the two electric planes, the lower being the guiding one. I know there has been a great deal of talk about other possible systems of supply. We have heard much of the charged rail using low potential currents supplied at frequent intervals by motor generators driven from a central station. Since we have discovered no conductors devoid of resistance, and the art of welding is not particularly applicable for railway service where moving contacts are a necessity, little credence need be given to any scheme of this character.

We hear again of the central rail supported on posts in wells between the tracks. A centre rail may be acceptable on a system like that of the elevated roads, but in ordinary railroad work any ditch intended to drain a track so as to keep insulators dry and keep snow away from them would probably so open the track that any moisture and frost would cause upheavals of serious character, and the cost of maintaining the track would be prohibitory. The use of snow machinery for keeping the track clear would be impossible, and anything underneath the cars is in the most exposed place for sustaining damage by defective rolling stock, and continually liable to all sorts of mechanical injuries and accidents with all the evil results of interruption of current, short-circuiting and stoppage of traffic.

Another system which has been proposed is that of conductors supported on posts alongside of the track and elevated but three or four feet above it. While not open to so serious objections in the matter of insulation as a centre rail system, its use is manifestly not to be thought of where there are crossings or switches. Even on a straight clear track in a hard climate there would be most serious trouble in the matter of clearing the tracks from snow, and at the grade crossings in this country gaps in the conductors would be too great for the contacts on a single locomotive to stand.

Some time ago I was requested on the behalf of a well-known financial man, whose enthusiasm as to the possibility of electric traction is well known, but who is withal a most practical railroad man, to inspect various railroad terminal tracks in this city with a view of, first, the substitution of the switching work now done by a part or all of the 1,800 or more switch engines by electric motors, and eventually the operation of suburban service.

Considering the railroads merely as they exist on a number of systems in Chicago, in the space of about two square miles, or more exactly, one and a half miles in length by three-quarters of a mile in breadth, it is no exaggeration to say that there is or will be not less than 75 miles of track, and switches and crossings, with their various combinations, almost innumerable.

I went over and over some of these tracks with the one thought in my mind—How can the current be delivered to the locomotives and how can the automatic signaling be done? And I was forced to the one conclusion to which it seems every man who makes the investigation must come, and that is that the overhead system of supply is the only one, but that it must be as substantial and thorough in character as that of any other part of the system, and that in view of the cost, such a system is only permissible where the number of units operated is large and continuous. These conclusions are not new, but they have been emphasized by the particular problem which is here represented.

Intimately connected with the question of conductors, and one of the most serious ones which has to be met by the electrician is of potential. The personal danger limit of continuous or ordinary period alternate currents is pretty well determined, and it seems generally admitted by constructors that the danger limit for the continuous current machine, with its commutator, is about the same as the personal danger limit.

Hence we meet with two dilemmas. If using continuous current motors, we are limited to a difference of potential per machine of 1,000 to 1,200 volts, and we can, so far as safety of the machines is concerned, probably go above this limit only by putting the motors in series, precisely as has been proposed for long distance stationary transmissions. If this is not done, then we have the introduction between the transmitting dynamo and the receiving motor of a motor generator system, another pet theory which is often suggested but which I unhesitatingly pronounce as so uneconomical as to be impracticable. If using an alternate current system, then converters must be used, either distributed along the line and supplying the working circuit or placed upon the locomotive to safeguard the motors. While the use of a converter under these conditions is not as objectionable as the use of a motor generator, it cannot commend itself as a very practical scheme, and certainly in view of the fact that no single phase, single alternate motor promises, up to the present, serious success. For the present, and, I think, for a long time to come, we must confine ourselves to the consideration of railroad problems, where continuous currents are used, and where the traffic between two points is sufficiently large to justify the investment in central stations and conductors which would be required for the operation of such a system. There are two methods of propelling trains electrically, one by following steam practice, that is by building a motor and hooking it to the head of a train, the weight of the motor being such as is required for the necessary power and traction when grades or slippery tracks are encountered. From all that has been developed up to the present, to get the control that is necessary and to build the machines safely, the electric locomotive will weigh nearly as much per horse power as the steam locomotive. This weight can be better distributed, but I do not think, if steam practice is followed on trunk line service, that there could be any very material reduction of weight of train.

The other plan is to have each car propelled by one or more motors. This would be the ideal system as far as propulsion goes, provided the electric motor was unlike all other mechanical apparatus, and that it never failed, and if a number of machines could be as well taken care of, cost no more and show as little depreciation as fewer machines of larger capacity operated as a unit. Should we ever arrive as we hope, to possession of a single circuit alternate current motor, then in view of the simplicity of its control, we may fairly hope for the distributed motor system. But here also the capacity and likewise the weight of the motor being determined by the total duty done, the weight of train limit would not be decreased, but rather increased. If, on the other hand, single units are used, the query naturally arises, what form would the electrical locomotive of the future take? I do not think this is by any means settled; undoubtedly gearless machines will be used, but whether they will be mounted directly upon the axle, or whether they will movably inclose the axle and be flexibly connected to it while their weight is carried on springs on the truck, or whether the motor will be carried on the truck frame and connected to the drivers by the ordinary coupling rods, are questions which will be determined eventually by the depreciation per car mile upon the motors, trucks and road-bed as well as by the speed to be attained. Whatever the method of mounting the motor is adopted, for reasonable weights and powers, a two axle truck will be used, but where large powers and weights are necessary, two such trucks will be coupled together so as to keep the weight upon each wheel within limits, and this will carry a cab containing the regulating and collecting devices, and so shaped as to offer the least resistance to air pressure and high speeds. I have never advocated the use of a connecting rod in transmitting the motion from an armature to a driving axle, but I think it fair to say in this respect that the so-called hammering effect on the rails, said to take place in the ordinary locomotive driver where the weights are counter-balanced, exists more in imagination than in fact, and that the chief trouble in the use of the connecting rod is the change of direction of its movement. Among the roads that are ripe for the electrical engineer, and on which in the near future I hope he will demonstrate he has a most legitimate claim, are the New York elevated and the Chicago elevated, the handling of the trains on the New York Central and Harlem roads below the Harlem river, the long talked of rapid transit road of New York, the Metropolitan underground road of London, the proposed tunnel roads in London, Paris and Berlin, and coming more immediately home, suburban service such as that of the Illinois Central railroad, a most ideal track for the electrical engineer, and last, and as it will prove one of the most important, the operation of the terminal and warehouse systems for the interchange of freight on the lines entering a city situated as is Chicago. Taking this last, we have here a definite problem, now performed in a more or less satisfactory way by steam service. It is a problem large enough of itself. It has little connection with electric trunk line service, and the present impracticability of the latter has little bearing upon the thorough practicability of the former. Eighteen hundred or more switch engines, many of them on duty twenty-four hours a day, a large portion of the time standing idle, puff their foulness into your overburdened atmosphere, because from 80 to 90 per cent. of all the freight that comes into the depots of this city ought never to come inside its limits, and would not, were there a practical way provided to transfer it from one railroad to another outside the city limits. It has been suggested, and it seems to me a most feasible plan, that there shall be established a vast system for interchanging freight on the various railroads by a great 6-track crossing belt road which shall form a common zone of transfer either by itself or in combination with freight warehouses or storage yards. Undoubtedly there are many difficulties in the way, but from an electrical standpoint there is absolutely no question but that such a system of belt line is practicable.

With such unsolved problems, such abundant fields, I deem it unnecessary now to attempt to build electric locomotives to pull trains of great weight 100 miles an hour, or to develop a system of conductors for trunk line service which is not possible for yard duty, or to consider a central station or track equipment for a duty never required. This problem is in a measure an experimental one, which being carried to a certain measure of success will clearly point the way for a future development and outline its limits. I may be pardoned perhaps if I take radical views in some matters of railroad practice. I have fortunately or otherwise been thrown into direct touch with all the larger work which is to be done in this country during the coming year, and it gives me pleasure to announce, what many of you know from the current news of the day, that there will probably be in operation in the United States within 12 months, not less than five locomotives varying from seven to twelve hundred H. P. and from 45 to 80 tons in weight. The character of the work done will vary. In that work which I am most concerned from a personal standpoint, a 700 H. P. electric locomotive will be built for experimental work, and to attempt to solve as far as may be the various problems which are involved in railroad practice in Chicago. If my judgment is followed there will be an experimental section of track in the form of a loop about 13 miles long with 18 miles of rail, and with every variety of single and double track construction, and simple and compound crossings and switches.

On this I hope to see erected such varieties of overhead construction as may be found best to meet the various kinds of service, and where the railroad problems on tracks jointly operated by steam and electricity can be developed in the most satisfactory manner.

On this track there will be not only this locomotive, but also one of equal rated capacity supplied by one of the larger manufacturing companies.

The duty demanded of these machines will be severe. They will be required to haul a train of not less than 450 tons, at 30 miles an hour, up a grade of 26 feet to a mile.

They will probably be required to develop their full rate of capacity at all speeds between 30 and 60 miles per hour, and if there is sufficient track room they will be driven at a speed of at least 75 and perhaps 100 miles per hour for short distances. The potentials used will be nearly double that at present obtaining in street railway practice.

A still larger problem, so far as power goes, although not in the variety of conditions which will have to be met, will be that recently taken for the operation of the belt line tunnel now being constructed in Baltimore to avoid the necessity of boat transfer at Locust Point. The duty of the motor will be to propel the train with engines coupled on, but not in operation, through a tunnel about 6,000 feet long.

The conditions require the motors, which will weigh about 80 tons and have a capacity of about 1,200, to propel a 1,200 ton freight train up a grade of 42 feet to the mile at a speed of 15 miles. Passenger trains of 450 tons weight must be regularly started from rest twice in the tunnel on this grade, and in an emergency the freight train must be started. The draw bar pull under regular duty, and when not starting, may be as high as 35,000 pounds.

Perhaps the traffic from New York to Philadelphia affords as good an example as any of what may be done on regular passenger service, provided the track is clear enough.

For this I some three years ago, and again in the *Forum* of September, 1891, outlined an electric express service, with a method of supply through a rod carried above the car and a return circuit through the rails and earth.

The current was to be supplied from one or more central stations, equipped with high-class triple expansion engines, driving multipolar dynamos directly coupled. What the electrical engineer, and the railroad man as well, need to know is whether the electromotive force required on the line, and the number of stations necessary would be prohibitory.

No attempt was made at an excess of speed, but I confined myself to the average speed of a mile a minute for a distance of 90 miles, and considered a through service only. I assume a total weight of copper of only about two-thirds that which exists on the long distance telephone line between New York and Boston. The trains were to be two car units, leaving every 10 minutes.

I found with these conditions the stations and potentials would be about as shown in the following table :

Station.	Miles apart.	Potential. 2 wire.	Potential. 3 wire.
1	—	3,600	1,800
2	45	1,800	900
3	30	1,200	600
4	22½	900	450

If the three wire system is used, that is, the rails as a compensating conductor between two trolley rods, then, with only two stations forty-two miles apart, it is seen that the potential is less than 1,000 volts, and this we undoubtedly we can handle.

I am not prepared to say that we may not use even a higher pressure, because I believe whatever is demanded in the interests of economy, all things being considered, will be used, but if we can reduce the potential to perfectly safe and reasonable limits by multiplying the number of stations, then those stations should be increased so long as the increase does not seriously affect the general expense of working. On a service of this character, where I have made the conditions distributed work, and the despatching of units at brief intervals, which conditions, I repeat, are absolutely necessary if we are to consider long distance transportation by electricity, such increase of stations as is advisable will not increase the cost of central station operation.

Such is the work before you, a work well meriting your best efforts, yet it is well to temper your enthusiasm with prudence.

Limit your attempts to the solution of those problems which will prove of practical benefit.

Do not chase rainbows. They are beautiful and poetic, but they have small place in the world's economies.

Remember that neither sentiment nor ignorance are winning cards, but lessened costs of operation for equivalent duty and increased returns on invested capital.

All this is said in no spirit of discouragement, for I yield to no man in my confidence in the future of electric traction. No new field is so rich, none more pregnant with great possibilities, but the growth of the work will be more expeditious and healthy if we separate the visionary from the real, the impracticable from the practicable.

A paper read at the General Meeting of the American Institute of Electrical Engineers, Chicago, Ill., June 7th, 1892, Vice-President Lockwood in the Chair.

THE MAGNETIC PERMEABILITY OF SPECIAL IRONS FOR ELECTRICAL PURPOSES.

BY MILTON E. THOMPSON, PERCY H. KNIGHT AND GEO. W. BACON.

During the last year or two there has been a marked change and improvement in the qualities of iron used in the construction of electrical machinery. It is now generally well understood that not only are the efficiencies of transformers, dynamos and motors increased by improving the quality of iron used, especially where this iron is subject to changes of magnetism and consequent loss by hysteresis, but also that the capacity of any given machine will be greatly increased by improving the quality of the iron. Thus, in the majority of dynamos, the capacity of the machine is limited largely by the heating of the armature, and the greater part of the heat developed in an armature is usually produced in the core. If the heat developed in the core can be reduced by improving the quality of iron used, enough extra current may be taken from the armature to balance this saving in the core. Again, in a dynamo or motor, the principal part of the weight of the machine is due to the field magnets. If the permeability of these field magnets can be doubled by improving the quality of the iron used for their construction, the weight of these magnets may be reduced in the same proportion. In many classes of dynamo machinery, as, for instance, railway motors and marine dynamos, this saving in weight and consequent increase in the capacity of a machine of given weight is of great importance. It has long been known that the magnetic value of wrought-iron is several times as great as that of cast-iron; but the readiness with which cast-iron may be molded to the desired shape for field magnets has kept it in favor for this purpose, in spite of its low permeability. A demand has arisen for a quality of iron which shall have high permeability, and still possess the quality of being easily cast into the proper shapes for field magnets for dynamo machinery.

Many attempts have been made to improve the quality of cast-iron, but for the most part with little or no success; and cast-iron stands to-day as a metal having nearly the same magnetic value for any and all qualities, though some slight improvements have been effected by some makers, as will be seen later. Attempts have also been made to produce castings from wrought-iron, and lately with fairly good success. This is accomplished in general by melting down scrap wrought-iron in crucibles, rendering it fluid by the addition of a small quantity of aluminium and pouring as with ordinary cast-iron. A great many extra precautions have to be taken to insure sound castings, and as a rule the castings made from this "mitis metal," as it is usually called, are rough and somewhat difficult to work on account of their roughness. These castings haves hown themselves to be of very much higher permeability than cast-iron, and have rapidly come into favor, especially in the construction of railway motors. Makers of steel castings have also been experimenting, with a view to producing a metal suitable for field magnets of dynamo machinery. Some of them have been very successful in this undertaking, and steel castings for field magnets are to-day an undoubted success. Manufacturers of sheet iron have also been trying to keep up with their fellow workmen in this line, and not a little experimenting has been done, with a view to the production of better qualities of sheet iron.

The writers having recently made, in the laboratory of Cornell University, quite a number of tests of samples of various special irons from different manufacturers throughout the country, it was believed that some of the results obtained would prove of interest to the general electrical fraternity. The method used for these tests was a modification of that used by Rowland, the samples being prepared in the form of rings, and wound with two sets of windings, a primary and a secondary. The magnetizing current was passed through the primary circuit and measured by means of a Weston ammeter. A ballistic galvanometer placed in the secondary circuit measured by its throw the magnetic induction due to any given change in the magnetizing current.

Two different plans were tried in taking the curves and found to give slightly different results. The first was the reversal method, in which the current was commenced at its maximum value and its direction reversed several times by means of a reversing switch, in order to bring the magnetism up to its full value, the secondary circuit being open during this operation.

The secondary was then closed through the galvanometer, the current reversed, and the induction measured by the throw. The current was then slightly reduced, reversed several times as before and the reduction again measured on reversing the current. In this way the magnetizing current was gradually reduced to zero, and from the data thus obtained curves were plotted with magnetic lines per square centimeter for ordinates and magnetizing force for abscissæ. In the other method, which was used for taking most of the curves and which gives the results nearly like those obtained by Hopkinson and which are generally accepted as standards for wrought and cast-iron, the sample was first completely demagnetized. In some cases this was done by simply reversing the current through the primary and at the same time gradually reducing it to zero. By this method, however, no samples except those composed of wrought-iron sheets were successfully demagnetized, which might suggest that the Foucault currents set up by the reversal of the magnetizing current tended to keep up the magnetization. An alternating current of both high and low periodicity was also tried without success, and it was only by finally heating the samples to a cherry red heat and allowing them to cool in air, so as not to change materially their softness, that total demagnetization was obtained. The sample being demagnetized, a small current was passed through the primary and the induced magnetism measured by the throw of the galvanometer. The current was then slightly increased and the deflection of the galvanometer again noted. And so on up to the maximum magnetizing force, the current being gradually increased step by step. The current was then reduced step by step to zero, reversed and increased step by step to a maximum equal to that of the other direction. This descending and reversal curve was taken simply to serve as a check upon the demagnetization and upon the accuracy of the observations of the ascending curve, and only the latter was plotted. Curves obtained from the same sample by the reversal method and by the step by step method were found to differ from each other by eight or ten per cent. at maximum values, the curves by the reversal process reaching the higher values. The magnetization of field magnets of dynamo machinery would probably reach higher values than that given by either of these methods of testing, for the iron, being subject to vibrations from the rotating parts of the machine, and being at the same time under a continuous magnetizing force, would no doubt reach as high magnetic values as could be obtained by any method of testing.

The results obtained are embodied in the accompanying curves, which present the results of tests on samples of iron from a dozen different companies, in many cases a large number of samples of the same kind of metal having been tested. Referring to these curves:—Fig. 1 presents curves of two samples of steel castings from two different companies. The curves show both samples to be of very good quality, sample No. 1 being slightly the better of the two. Fig. 2 shows two

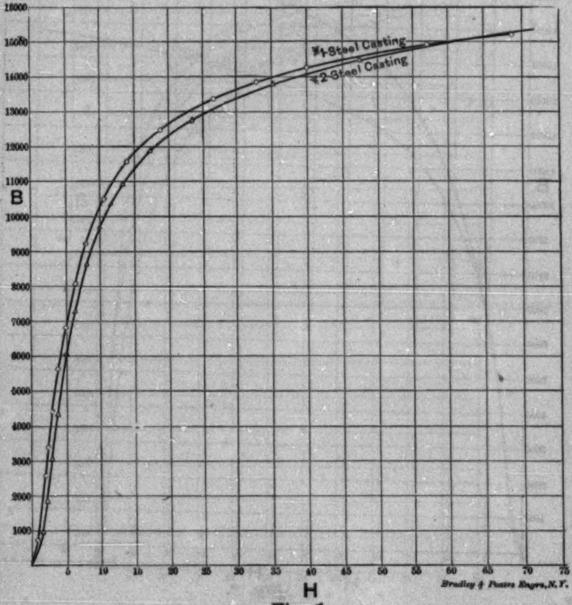

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Fig. 1.

more curves of steel castings, No. 3 being an average curve of about a dozen different samples of steel castings furnished by one maker, and No. 4 being an average curve of several samples of steel castings furnished by another company.

The different samples represented in curve No. 3 did not differ very materially from one another, and there was but a very slight difference in the samples represented by curve No. 4. Curves Nos. 1, 2 and 3 were taken by the step-by-step method, while curve No. 4 was taken by the reversal method, which will account for the high value of this curve, in a measure, though it is very probable that the quality of the steel repre-

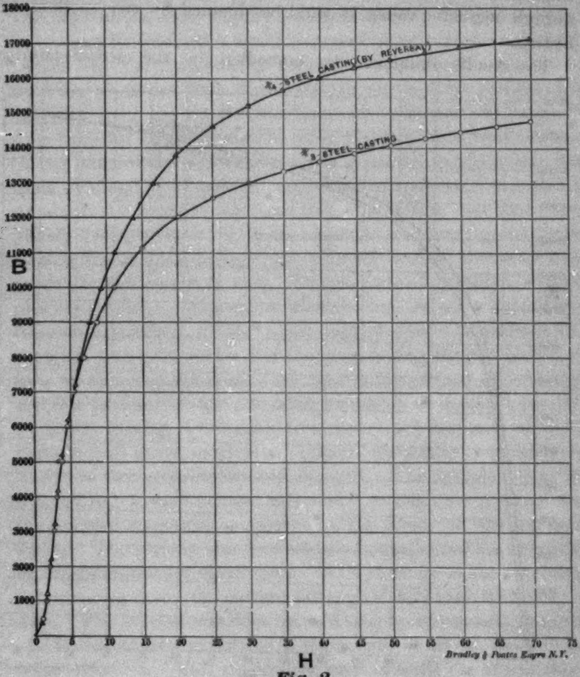

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Fig. 2.

sented by No. 4 was somewhat better than that of the other samples. The curves on Fig. 3 were all taken by the reversal method. Curve No. 5 is the average of two different samples of wrought-iron castings from the same maker, the two samples prepared at different times and from different pourings showing almost identical qualities. The magnetic value of mitis metal, as shown by curve No. 5, differs very little from that of steel of good quality, as shown by curve No. 4. Curves Nos. 6 and 7 are from two samples of sheet steel of somewhat peculiar manufacture, inasmuch as they are said to contain no manganese, that element being perhaps the most objectionable impurity in mild steel, so far as its magnetic properties are concerned.

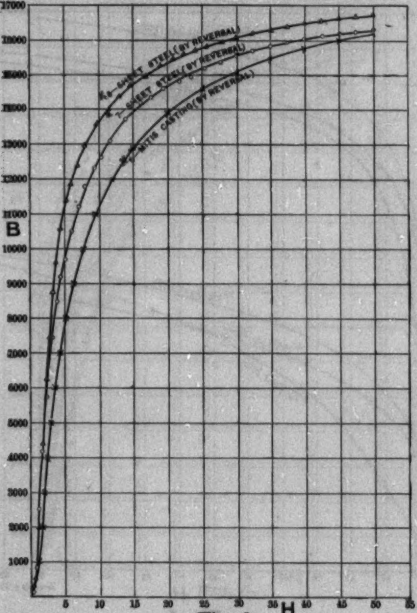

Fig. 3.

The two samples from which the curves were taken were made as an experiment, and by slightly different processes. They show excellent magnetic values, especially No. 6, and seem to indicate that sheet steel may some day come into extensive use for armature cores, etc. Curves No. 8, No. 9 and No. 10 of Fig. 4 are from three different samples of sheet iron used for armature cores and transformers by three well known electrical companies. Neither of these is up to the values given by Hopkinson for good wrought-iron, but the difference in the method of taking the curves may account in a measure for their being lower. These curves were taken by the step by-step method. Curves No. 11, No. 12 and No. 13 of Fig. 4 are from three different samples of cast-iron, used by three electrical companies for field magnets of dynamo machinery. Sample No. 11 is an ordinary quality of cast-iron and differs from the values given by Hopkinson for cast-iron by only an inappreciable quantity. No. 13 is rather peculiar, in that

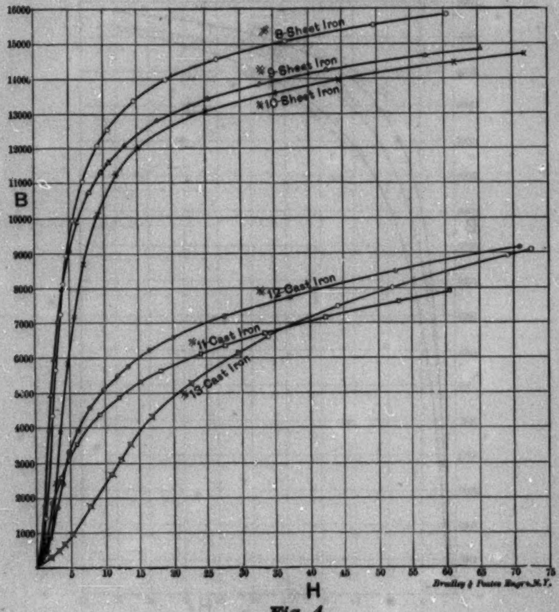

Fig. 4.

for high values of magnetizing force it is slightly better than ordinary cast-iron, while for low values it is much poorer. Its magnetic properties are more like those of hard steel than of cast-iron. No. 12 shows a metal whose magnetic value is from 10 to 15 per cent. higher than ordinary cast-iron, the curve still keeping its same general shape. This sample would seem to indicate that there is such a thing as improvement in the magnetic qualities of cast-iron. Curves No. 14 and No. 15 represent two different samples of wrought-iron, used by two electric companies for field magnets. These curves were taken by the reversal method, and indicate for the metals a value considerably below that of the best wrought-iron.

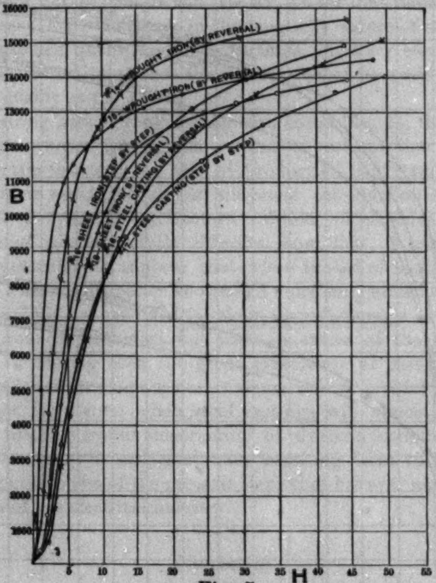

Fig. 5.

Curves No. 16 and No. 17 are both from the same steel casting, and show the difference between the step-by-step method and the reversal method. Curve No. 16 is by reversal, and No.

17 step by step. Curves No. 18 and No. 19 are both from one sample of sheet iron, No. 18 being taken by the reversal method, and No. 19 by the step-by-step. A similar difference in the two methods is indicated by these two curves. Fig. 6 is an attempt at a comparison of some of the more interesting of the curves mentioned above. Curve No. 20 is Hopkinson's curve for wrought-iron. Curve No. 23 is an average curve for steel castings as made by four different companies. Curve No. 21 is a good quality of steel casting by the reversal method, and No. 22 is an average of mitis metal, by the same method. Curve No. 24 is Hopkinson's curve for cast-iron, and No. 25 is an extra quality of

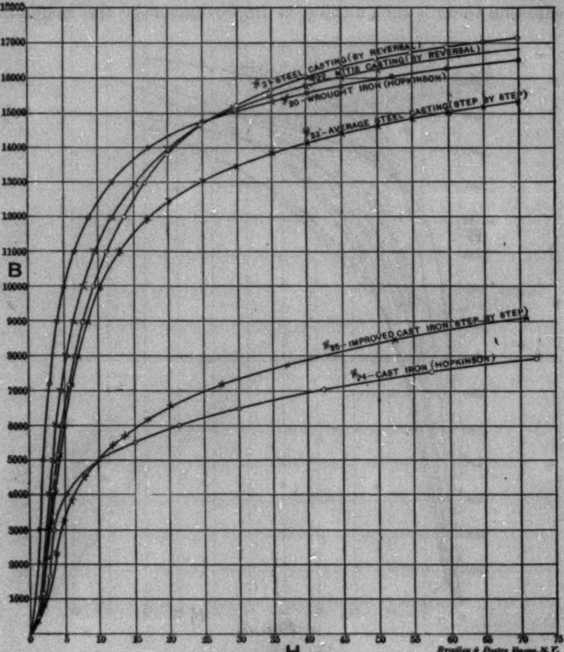

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Fig. 6.

cast-iron, which would seem to show that there is still a chance for the standard of cast-iron to be raised. A comparison of curves No. 20 and No. 23 shows that the magnetic value of the average steel casting is only about eight per cent. below that of the best wrought-iron, and from curve No. 21 we might infer that it would be perfectly safe to use for the best steel castings the magnetic values given by Hopkinson for wrought-iron. Comparing curves No. 21 and No. 22, we see that there is practically very little difference in the magnetic values of steel castings of the best quality and mitis castings, and there seems to be no reason for believing that the former cannot be made of quite as good quality as the latter.

Comparing both of these metals with curve No. 24, their magnetic values are so far superior to that of cast-iron that it seems quite certain that only the cost of production can stand in the way of their rapid and almost universal introduction for field magnets of dynamos and motors. When we consider that the cost of steel rails is but little different from that of cast-iron, it seems reasonable to suppose that when the art of making steel castings comes to be better known and more fully understood, and better facilities are provided for their manufacture, such castings can be produced at a cost but little in excess of that of cast-iron. Considering then the great superiority of cast-steel over cast-iron for electrical purposes, it seems safe to predict that the day is not far distant when steel castings will almost entirely replace cast-iron in the construction of dynamo machinery, and the improvement in such machinery resulting from this change will be perhaps one of the greatest steps that has ever marked the progress of the electrical industry.

DISCUSSIÓN.

THE CHAIRMAN [Vice-Prs't Hering]:—I am sure that we all feel very much indebted to Dr Nichols for reading, or I should say, for explaining and at the same time discussing this paper in the very interesting way in which he did. The paper is open for discussion. There are doubtless here many who have worked in this field, and we would be very glad to hear from them.

PROFESSOR CARHART:—I am very much interested in the paper and in the results. Particularly because I made some similar experiments two years ago, and secondly because some of my students are now carrying on experiments similar to these. Nearly two years ago I tried the relative permeability of sheet-iron of several varieties; sheet-iron and mitis steel, and the results entirely confirmed those given in the paper. I found that certain varieties of mitis steel have higher permeability than sheet-iron. I find quite a decided difference in that direction. I should like to ask Dr. Nichols about the hysteresis curves of the steel as compared with the sheet-iron.

Dr. Nichols:—I will say that the hysteresis curves have been obtained in some of these specimens, but that work is still going on I should like to say also, in addition, as a contribution to the discussion, that a third set of measurements, carried on by another observer in our laboratory, was entirely confined to the question of open hearth steel as we get it in the form of boiler plates and armor plates made for the government, the test rings being cut directly from such plates. It was found that these open hearth steels, as manufactured for boilers, and for the armor for vessels, show precisely the same qualities as the average of the steels, treated of in the paper under discussion. The magnetization curves of two such open hearth steels, tested by the obsever of whom I speak (Mr. F. W. Throop) are given in the following dia-

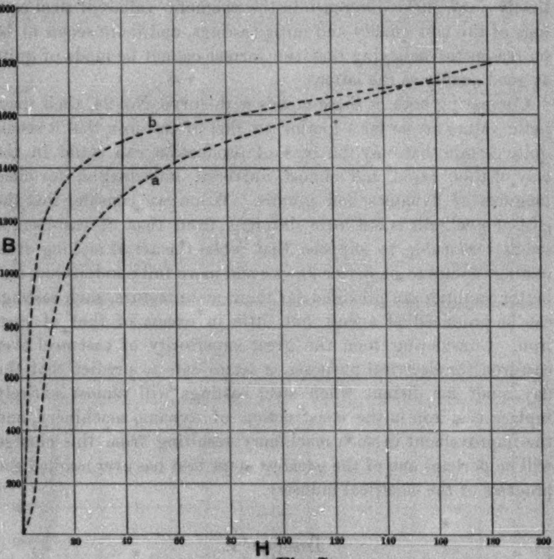

Fig. 7.

gram. Curve *a* was obtained from a ring cut from a government armor plate, *b* from a similar ring of fire-box steel.

Dr. Emery:—Mr. President, my statement yesterday that the permeability of steel at high magnetizations was greater than that of wrought-iron, was based upon statements of Mr. Steinmetz, who has done a great deal of good work in this direction, and upon tests of experimental motors with steel lamina in the armature, which gave very excellent results. I will ask Dr. Nichols to state for the record whether the curves based on Hopkinson's experiments are the actual experimental curves or those presented by Thompson, which show the averages of the ascending and descending values.

Professor Nichols:—I am not sure that I can answer that in the way it is put for the purpose of a record in this matter, for I am not quite positive of it. I think it is the ascending curve of wrought-iron. The intention, of course, was to get a Hopkinson curve which was in every respect comparable to those just described.

Professor Jackson:—Mr. President, I did not hear this paper, but I do not see anything on the sheet of curves regarding the chemical analysis. I have done some work of this kind, and have found that the amount of carbon in steel castings and also in rolled steel has a great deal to do with the permeability, and also a great deal to do with the form of the curve of magnetization. By controlling the amount of carbon, and also by controlling the amount of manganese (these being the common impurities which have the greatest effect on permeability, we are enabled to vary the form of the curve of magnetization considerably. If Dr. Nichols has an opportunity to carry out a consecutive series of experiments, determining the amount of carbon and manganese in mild steel, of which he makes permeability tests, he will get some very useful results, far more useful indeed than the permeability tests can possibly be without the chemical comparisons.

The variation of the magnetic properties of iron and steel by aluminium alloys seems to be due simply to the effect of aluminium as a flux. As a flux it changes the proportion of carbon; and hence may be used to advantage, though its presence is a disadvantage where its fluxing properties are not required. The purer the iron is, the better the magnetic result seems to be, *i. e.*, the higher the curve of magnetization will run before it makes a bend. By the introduction of aluminium we reduce the magnetization at the point of saturation (just exactly as would be the case by the addition of carbon or manganese), but aluminium controls the carbon to a certain extent in mild steel, and it has a marked effect in some cast-iron. In cast-iron the control of silicon seems to be of about equal importance with the control of aluminium and manganese.

Mr. Bradley:—Professor Jackson, have you analyzed cast-iron?

Professor Jackson:—I started in on cast-iron, but did not get very far, so that I cannot give any thoroughly good results. A number of test pieces of different kinds of cast-iron were made. One piece was made of very hard cast-iron, an iron that chilled badly, and which it was almost impossible to work either on a lathe or with a file. and its magnetic properties were very poor. By properly alloying with aluminium when the iron was cast, we succeeded in making the carbon come out in the form of graphitic carbon instead of combined carbon, giving a good soft iron with quite fair magnetic properties. It seems impossible however to produce quite as good magnetic properties with aluminium present as first-class *graphitic* iron shows without the aluminium.

A paper read at the General Meeting of the American Institute of Electrical Engineers, Chicago, Ill., June 7th, 1892. Vice-President Hering in the Chair.

NOTES ON WIPING CONTACT METHODS FOR CURRENT AND POTENTIAL MEASUREMENTS.

BY PROF. BENJ. F. THOMAS.

In the discussion on Mr. Milton E. Thompson's paper on "A Study of an Open Coil Arc Dynamo," read before the Institute on the 21st of May, 1891, Dr. Geyer referred to some work done at Stevens Institute by the writer, in which work the wiping contact method seems to have been used for the first time. The following brief account of the work is presented at the request of several members of the Institute.

In the latter part of July, 1880, President Morton requested the writer to determine the power required to maintain the arc of a Brush arc light, supplied with current by a Brush dynamo machine.

Ammeters and voltmeters were then unknown, and among the electrical instruments in the Stevens laboratory the most convenient for the purpose seemed to be a tangent galvanometer for the current measurement, and a condenser and Thomson galvanometer for potential. The condenser was a one-third microfard standard, and the galvanometer 7,000 ohm, four-coil astatic, both made by Elliott Bros. As every one now knows would be the case, trouble was encountered the moment the potential readings began. The condenser being first connected to the two carbons, and then discharged through the galvanometer, the successive throws of the needle were found to vary widely, and what seemed at the time most curious, *the throws were sometimes to right, and sometimes to left.* A little reflection showed that while varying throws in one direction might be due to varying resistance of the arc, throws in opposite direction could only be produced by an actual reversal of the potentials of the two carbons. This, of course, led to the conclusion that the potential difference at the terminals of the dynamo must vary with the angular position of the armature as it revolved. Wishing to know the character and magnitude of the variation, the writer devised what is now known as the wiping contact method, the natural and only expedient for the purpose.

The condenser was joined, through the usual discharge key, to the terminals (or other points) of the dynamo, by two insulated wires. At the dynamo one of these wires was cut, and the cut ends attached to a pair of well insulated flat springs of metal, mounted on the outer end of a radial arm, the latter fastened to a horizontal rod, coaxial with the armature. A well insulated piece of metal was fastened radially on the face of a disk attached to the armature shaft, and the two brushes set so that they were lightly touched by the radial strip at each revolution of the armature. A pointer attached to the horizontal shaft carrying the

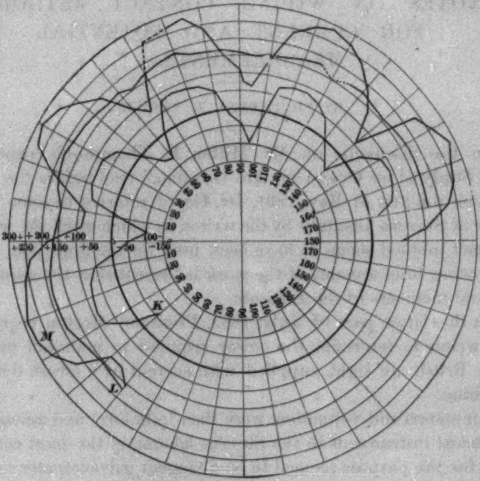

Fig. 1.

radial arm and contact springs, moved over a divided circle on the support of the arm, so that angular positions could be read. The contact device thus acted as an open circuit key, the closing of which, and the consequent charging of the condenser, could only take place when the armature was in a definite position. The process of obtaining the curve of potentials through a revolution of the armature is now so familiar that further description is unnecessary.

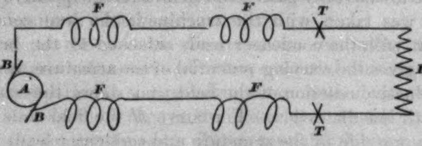

Fig. 2.

In carrying out the examination of the machine, a non-inductive resistance of german silver wire was prepared to avoid complications from the action of the arc lamp. When this was used as a load for the machine, the throws of the galvanometer changed at once from the erratic behavior observed at first, to the beautifully regular and uniform character now so familiar to all who have used the method under proper conditions.

Fig. 1 is a copy, to scale $\frac{2}{10}$, of the original curves of measurements made August 4. The inner circle gives the angular readings of the circle over which the pointer moved. Deflections of the galvanometer, on discharging the condenser through it, are plotted radially, at the angles at which they were obtained, the heavy circle being taken as zero. The scale is .2 volt per division. Curves *K* and *L* were obtained with the machine connected as in Fig. 2, in which *A* is the armature, *B B* the brushes, *F F* the four field coils, *T T* the terminals of the dynamo, and *R* the non-inductive resistance. The curve *K* gives the deflections obtained when the condenser leads and wiper were joined at *T T*. It will

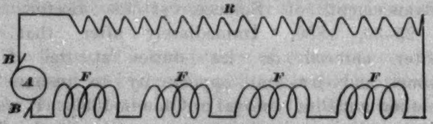

Fig. 3.

be observed that the curve is symmetrical, giving eight maxima and eight minima per revolution. The minima are below zero, and explain the reversed deflections observed in the first work on the arc. The resistance *R* being non-inductive, *K* is also the curve of current strength, to a different scale, and shows that the machine was an alternating current machine, although the excess of positive area over negative shows an average positive current. Curve *L* was taken with the machine in the same condition as before, but with the condenser leads attached to the brushes at *B B*. It gives the varying potential of the armature, as modified by the inductive reaction of the field, and shows the same symmetry, with less distortion. For curve *M* the field coils were all thrown on one side of the armature and condenser leads attached to *B B*, as in Fig. 3.

To ascertain the effect of an inductive external resistance on the terminal potentials of the machine normally connected, the connections used in obtaining curve *K* were repeated, the only change made being to insert the six coils of the large Stevens electromagnet with the non-inductive resistance *R*, adjusting *R* to get the same current strength used in getting curve *K*. The resulting curve showed the same characteristics as *K*, but much magnified, deflections ranging from — 300 to + 550. Many other curves were taken, with the machine self-excited and separately excited, on various loads and on open circuit, but the results, though interesting, are not of sufficient importance now to occupy your time. The most interesting curves and results were presented as a joint paper by Prof. Morton and the writer, and read by Prof. Morton before the American Association for the Advancement of Science, at its Boston meeting, August 25–30, 1880. Immediately after that meeting the writer entered on his duties at the University of Missouri, and had no opportunity to resume the use of the method until his removal to Ohio in 1885. It was used in that year by his students in an examination of the Thomson-Houston arc dynamo, substituting a Thomson portable electrometer, with Leyden jars to increase its capacity, for the condenser and galvanometer. Since then the method has been used in this laboratory on various apparatus, and in various ways, but it is needless to recount the details.

Our experience with the method leads us to prefer the Thomson portable electrometer and Leyden jars, to all other instruments for high potential curves. The electrometer is dead beat, is a zero instrument, and is easily and quickly set. As usually made, its constant is from two to three volts per division, and one can easily set to within that amount. The error resulting from an improper reading, say one division wrong, would be of course serious in reading low potentials, but these readings only occur on nearly straight parts of high potential curves, and might be entirely omitted without serious error.

Next to the portable electrometer, we prefer the Thomson electrostatic voltmeters. For low potential curves nothing has yet proven superior to the potentiometer method as used here, and described by the writer at the American Association meeting at Washington last August. The potential difference to be measured is balanced against that produced by a battery in a resistance as in Fig. 4.

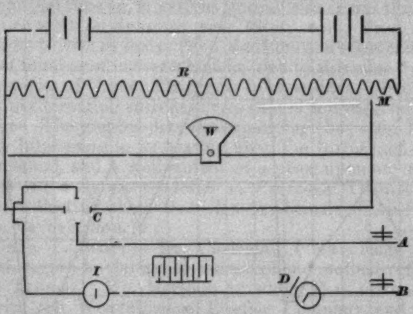

Fig. 4.

In the figure, *R* is a resistance of some convenient form, with which contact may be made with a movable terminal *M*, or which may be varied between or beyond the terminals of the Weston voltmeter *W*. From these terminal wires leads through a commutator *C*, a sensitive galvanometer or telephone *I*, and the wiper *D* to any points *A B*, whose potential curve is desired. The whole being in operation, and *C* properly placed, *M* or *R* (or both) is adjusted until *I* shows no current, when the potential difference existing at *A B* is read off directly in volts on the voltmeter *W*. The use of the telephone is due to R. D. Mershon, a graduate of the university, who described the method in the *Electrical World* of August 29, 1891.

Fig. 5 is a part of the potential curve recently taken from a Brush open coil motor, run as a dynamo on an inductive load. One-fourth of the complete curve is shown, and the parts not given are quite symmetrical with this. No "smoothing" is applied, but the observed points are joined by straight lines. The sharp angular form shown is due to the fact that readings were taken at intervals of five degrees, except between 25 degrees and 35 degrees, where readings were taken at each degree, to determine the character of the sharp variation between 30 degrees and 35 degrees. The change of 130 volts which occurs between 32 degrees and 33 degrees is unique and noteworthy, yet it is perfectly definite and characteristic, and has been repeatedly observed under like conditions. The use of ammeters and voltmeters or galvanometers in power measurements on such machines must, as pointed out in the paper of 1880, lead to incorrect results.

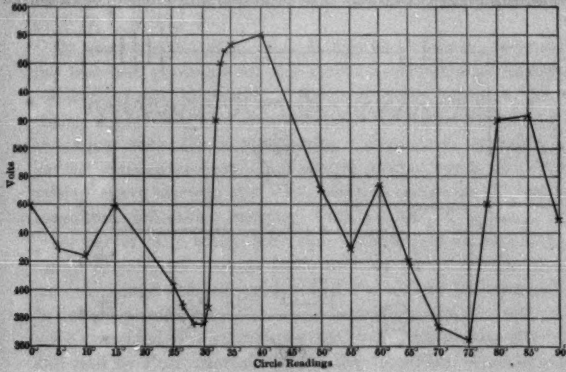

Fig. 5.

In Mr. Thompson's paper referred to above, reference is made to Joubert's use of the method in 1881, and in the recent edition of Thompson's Dynamo-Electric Machinery, it is described as Joubert's method. In view of the work here described, and its presentation before the American Association in 1880, it seems proper to claim the method, which in the hands of Dr. Duncan, Professor Ryan and others, has led to such valuable results, as an American method.

DISCUSSION.

DR. NICHOLS:—Mr. President, this is a subject in which I feel very much interest. It seems to me that we owe Prof. Thomas an expression of thanks for taking the trouble to revive the ancient history of this particular method which has grown to be of such importance. At the time when these measurements were made, they were so little appreciated as to have attracted no attention. The paper, as we know, was presented at the American Association for the Advancement of Science, and was regarded as a curious thing; and a dynamo itself at that time was regarded as hardly worthy of study in this country. So far as I know the history of this matter, the next place where this method crops out was in 1884, in Philadelphia, where it began to be seriously considered, and where it was put into practice, or at least considered one of the ways in which dynamos were to be studied. And certainly from that time on, in various laboratories, it was the method for practice work on dynamos, year after year. Prof. Jackson himself, who is with us to-day from Madison, doubtless can remember a great number of curves which he took by this same method in the laboratories of Cornell University in those days. The application of it to alternating current curves should doubtless be credited to Joubert. The graphic data accompanying the paper, I think is extremely interesting in its bearing upon the history of this important method, and is well worthy of a place upon our records.

[A vote of thanks was tendered to Professor Thomas for his efforts in collecting these facts for the benefit of the Institute, and upon its invitation.]

MR. CHAS. F. SCOTT:—Mr. Chairman, I may make a statement with regard to the use of the telephone method referred to by Prof. Thomas. Mr. Mershon, to whom that method is due, is now with the Westinghouse Electric Company, and has used the method in the laboratory work of the company in taking curves of electromotive force and current from alternating machines. The telephone is an extremely delicate instrument, and as we all know, is very liable to give undue prominence to sounds that are not wanted. In using the telephone we found that various sounds from the dynamo engine room could be distinctly heard, but the click of the contact is peculiar and can easily be detected and the sliding resistance can easily be adjusted so as to eliminate the sound of the contact, showing that the proper adjustment is made although other sounds may still be heard. Repeated settings can be made with very great accuracy. On one occasion a gentleman who was not accustomed to making electrical readings was invited to make a number of settings. I took the readings on a Weston voltmeter, and they ran something like this: 76.9, 76.8, 76.9, 76.9, and so on, showing that the method is susceptible of very great accuracy. It is a practical method, and can be readily and rapidly applied.

PROFESSOR JACKSON:—Mr. President, I think in the fall of

1885 I first learned of a similar method when I went to the laboratory at Cornell University. We then had several machines arranged for very careful examination in this manner: One was the old Ball dynamo, and I remember going through that machine with a great deal of pleasure a number of times. I have no doubt from the results that have come from Cornell since those days, that they have used and explained the methods many times. The method is undoubtedly one of the most satisfactory that can be used for studying the action of a dynamo.

The portable instrument that Dr. Nichols spoke of yesterday for taking somewhat similar curves, I am very much interested in, and trust that the subject will be more fully developed in the laboratory at Cornell. Such a portable instrument must be the natural outcome of the careful study that is now being made of the magnetic fields of dynamos, and if it can be put into commercial shape it will be practically invaluable to dynamo manufacturers.

Professor Carhart:—Mr. President, it would be well to have a little corroborative testimony of what Professor Thomas has stated. It does not go back to 1880, but I remember very distinctly in 1884, in Philadelphia, Professor Thomas and I were examining different machines, and the reference to this Ball machine revives it very strongly. We came in the vicinity of that, and I recollect Professor Thomas expressed a very ardent desire to explore the region around the armature of that machine, and that accounts for his interest in the subject, because he had already been practicing this method.

A paper read at the General Meeting of the American Institute of Electrical Engineers, Chicago, Ill., June 7th, 1892, Vice-President Hering in the Chair.

A LIFE AND EFFICIENCY TEST OF INCANDESCENT LAMPS.

BY PROFESSOR B. F. THOMAS AND MESSRS. P. MARTIN AND R. H. HASSLER.

Much has been written and said concerning the efficiency of the incandescent lamp, and of the relation between its efficiency and the length of life to be expected from it. High efficiency lamps have their advocates and low efficiency lamps have theirs. It is pretty well understood by those whose work has put them in a position to appreciate the facts, that the question as to the choice of the lamps which will have the best influence on the profit and loss account of the consumer, depends on other things than first cost, efficiency and life. It is understood by such persons that the cost of producing the current supplying the lamps, and the steadiness of voltage maintained on the lines are important factors in the choice; the high efficiency, short life lamp being best under conditions of steady voltage and expensive current, and the low efficiency, long life lamp where current is cheap or regulation poor.

The general public, however, does not understand the question so clearly, and unfortunately that part of the literature of the subject which is found in the advertising columns of the electrical press, does not tend to relieve the embarrassing ignorance of the buyer. Statements of the marvelously long life of certain lamps are followed in a week by ludicrous accounts of searches for them with a lantern, and of the imminent peril of the country, arising from the exhaustion of our coal supply by the running of such lamps. Again we are startled by statements of the wonderful results attained by lamps of some certain make, as indicated by the number of such lamps which can be maintained at full brilliancy by the expenditure of a horse power. But immediately it is said that the insane asylums of the country are full of station managers and stockholders, whose cases have become hopeless through contemplation of the number of such lamps bought during the year. There is as great divergence between the published claims of lamp makers, and the statements of the experience of station managers. Makers generally say that they will guarantee 600 hours life for their lamps, and from 10 to 14 or more lamps per horse power, but some station managers insist that they do not get an average of 400 hours life.

Lamp makers have, from necessity, made a study of their own lamps, and of those of other makers also, as to efficiency and life, and they know, better than any one else, the character of the incandescent lamp of to day. But naturally they, if they say anything on the subject, tell about the good points of their own goods, and the bad points of others, and, as naturally, the buying public say that all their statements are questionable. Much good work has been, and is being done in college laboratories on lamp efficiencies, and the results are published and accepted, but on the equally important question of life the laboratories are silent. The tests of the Franklin Institute stand as a model piece of work, but that work is now eight years old. Farther, the lamps tested at that time were furnished by the makers themselves for the purpose of test. Believing that the public would be interested in the results of a test of commercial lamps, as to both efficiency and life, the writers have carried out such a test and give the results in this paper. The test was made at the Electrical Laboratory of the Ohio State University, Columbus, O., and all expenses incurred were paid by the trustees.

Considering it of prime importance for our object that the lamps tested should be of the same character as those usually sold to consumers, we bought some lamps of regular dealers, as in the usual course of business, and obtained others from lots supplied for central station use. Lamps of the following makes were obtained, no attempt being made to get lamps of any given efficiency of any make: A. B. C., Beacon, Columbia, Economic, Packard, Pennsylvania, Standard, Thomson-Houston, Edison, Perkins, Sawyer-Man, and Steuben. In ordering (through persons purchasing for us in Chicago and New York) we called for ten 110-volt lamps of 16 c. p. in each case, but on receiving the lamps we found three lamps out of 10 of one make labeled 111 volts. The three were excluded and seven put in, but another lot of 10 of the same make, correctly labeled, was obtained. Ten of another make were received with the original marking, 111 volts in ink, changed by pencil to 110 volts. They were also excluded, and a new lot of 10 of that make, correctly labeled, obtained. Ten Sunbeam lamps were divided into two lots of five each, evidently supplied as a sample lot, and were excluded. We could not get others of that make in time to enter the test. There were, therefore, in all 127 lamps, labeled 110 volts and 16 C. P., of 12 well known makes, 17 lamps in two separate lots of one make, and 10 each of 11 different makes.

For convenvience in making the photometric measurements, the lamps were mounted vertically in horizontal rows of 20 on a light vertical rack, six feet high and 20 feet long. The rack was made in the form of ordinary shelving, having five vertical pieces of pine, four inches wide and one inch thick, and four horizontal strips of the same width, the four inch face being horizontal. The whole was rigidly braced diagonally, suspended by strong cords passing over pulleys in the ceiling and counterpoised by weights. This arrangement enabled us to bring each row of lamps to the level of the photometer described below, by raising or lowering the rack, and definiteness of position was secured by spiking to the brick wall of the photometer room two 2 by 4 inch vertical guides, against which the rack was pressed, and fastened each time by stout inclined pins, passed through the diagonal braces into holes bored in the guides.

A system of positive mains was arranged by fastening a No. 2 wire along the face of each horizontal strip in the rack, and joining them at the middle of the rack by a vertical No. 0 wire. A set of negative mains similarly made was placed on the opposite side of the rack. Flexible okonite cable of equivalent section joined the No. 0 vertical wires to the ends of the dynamo mains, fastened to the wall, which point is called the centre of distribution. Porcelain keyless wall sockets were screwed in place on the horizontal rack strips, and joined to the mains on each side by No. 16 wire. All connections were well soldered.

A Thomson centiampere balance provided also with a resistance for obtaining standard potential readings was used as a standard for current and potential. Its constants had been repeatedly checked by a voltmeter, and by Clark cells, and found correct and constant to within one part in one thousand. Two Weston milliampere meters and two Weston voltmeters were used in the current and potential readings on the lamps. The ammeters were graduated to 10 milliamperes, readable to one milliampere, and the voltmeters in volts, readable to tenths. The Weston instruments were compared with the Thomson balance at intervals, and found correct, or affected by a small, constant error, which was applied as a correction.

Current was supplied to the lamps by a 200-light Thomson-Houston dynamo, of the spherical armature type, which proved to be an excellent machine for the purpose. Owing to the variable speed of the engine, due to varying load and steam pressure, a special automatic regulator was devised and applied to the dynamo, to keep the potential at the centre of distribution constant. This was effected by causing a small motor to slide a contact piece along two vertical german silver wires joined to an extension of the field coil of the dynamo, a current being sent through the motor in one direction or the other as needed, by relay points, the relays being controlled by an indicator attached to wires leading to the centre of distribution. As used, the combination proved very satisfactory, keeping the potential difference at the centre at 110 volts, within a limit of ¼ of a volt either way. It could readily have been set to a smaller margin, but that was not considered necessary.

For the candle power measurements a two candle power Methven screen was used, the gas being passed through a Methven carbureter containing pentane. Its constant was checked by standard candles. Three 32-candle power Edison lamps were used as working standards. Each was checked at intervals by comparison with the Methven screen on a 100-inch photometer bar, a slide wire resistance enabling us to adjust the current through the lamp to a definite strength, which was always the same for a given lamp. The currents used were such as to develop about 18 candle power in the standard lamp. Instead of the usual Bunsen or Leeson disk, we used a Lummer-Brodhun prism, made by Schmidt and Haensch. It gave us much better satisfaction than the best Leeson disks we have been able to obtain.

In front of the test rack and parallel with it was placed a bench with a guiding rail, along which a Weber portable photometer was arranged to slide, so as to be placed successively in front of each lamp in a row on the test rack. Instead of the little benzine standard lamp which was removed, a 16-candle incandescent called the reference lamp was placed at a distance of about 800 centimeters from the end of the tube to which the bezine standard is attached. In taking the candle power of the lamps under test, the photometer was first placed in front of one of the working standards. By means of a conveniently arranged slide wire resistance, the current through the standard lamp was accurately adjusted to the strength used with it, when its candle power was measured on the standard photometer against the Methven screen. Care was taken that the adjustment of current was right when the measured potential at the terminals of the reference lamp was just 110 volts. Readings of the standard lamp against the reference lamp being taken the photometer was then moved in front of each lamp under test, and readings taken of each against the reference lamp. The candle power of each lamp was then found by comparing these readings with those obtained opposite the standard lamp. The reference lamp being of the same character as those under test, and connected to the same mains, the slight changes in potential allowed by the regulator affected alike the candle power of the reference lamp and the test lamp being read, allowing us to take readings more rapidly than we could have done had it been necessary to adjust and hold the potential at 110 volts.

Before the duration test began, the mean horizontal candle power of each lamp was determined in the usual way, and the ratio of this mean to the candle power found when the plane of the ends of the filament was perpendicular to the photometer bar was found, and used as the distribution factor. The lamps were placed on the rack in the same position with respect to the Weber photometer, at a distance of about one meter from its plate, the exact distance of each lamp being measured and used in the calculations on it. All results are expressed in terms of mean horizontal candle power. The unavoidable delays encountered in starting the test made it impossible to determine the spherical distribution without cutting short the time of run which we had set as desirable, viz., 1,200 hours. The duration test was begun on March 17, and it was necessary to run night and day, Sundays excepted, until the night of May 14, to complete the 1,200 hours. The run began with 77 lamps on the rack. The remaining 50 lamps were received later, and placed on the rack with the others 187 hours after the start.

As the methods used involve nothing particularly new, and have been fully described in print many times, we have consid ered it unnecessary to do more than state the general outlines, as above.

The object we had in view in undertaking the test was, to determine the character of the incandescent lamp as a commercial product, not as a scientific instrument. The number of lamps of each make taken was, of course, too small to base a fair judgment on as to the absolute or relative merit of the lamps generally sold by the makers. For that purpose some hundreds of lamps of each make, taken from different lots, would have to be tested, which it was of course impossible for us to do. For this reason we have considered it unfair to the several makers concerned, to give their names in connection with the results on their lamps, but have instead designated each make by a letter, and each lamp by a number. We believe, however, that the average of the results of all makes may be taken to represent fairly well the character of the average commercial lamp.

The results of the test are given in the following tables, the values given being in each case the average for a given make of lamp. Table I. gives in the second column the average value of the distribution factors found for the 10 lamps of make "A," and the average value of the mean horizontal candle powers for those lamps at intervals after the beginning of the duration test, expressed in hours by numbers in the first column. The third column gives corresponding values for the lamps of make "B," and so on. Table I. includes the lamps started March 17. Table II. contains similar data for the lamps started 187 hours later.

Table I.

Lamp.	A	B	C	D	E	F	G	K
Distribution factor	1.05	1.07	1.01	1.11	.989	1.01	1.01	1.02
Hours from start,	C. P.	C. P.	C. P.	C. P.	C. P.	C. P.	C. P.	C. P.
0	16.0	13.1	14.1	16.1	15.9	14.2	17.1	17.0
93	15.0	12.4	14.8	16.1	13.7	12.1	15.9	16.1
233	13.65	12.4	14.9	16.0	13.1	12.9	16.4	16.5
324	12.2	11.0	14.8	13.9	10.9	10.8	14.5	13.9
392	11.7	10.5	14.0	13.2	10.5	10.4	13.6	13.4
441	11.3	10.2	14.2	12.7	10.1	10.2	13.3	13.1
537	10.9	9.9	13.6	12.3	9.9	10.0	13.0	13.0
637	10.7	10.1	13.0	11.9	9.5	10.1	12.4	12.4
785	9.4	9.2	12.6	10.5	9.1	9.1	11.4	10.9
833	9.1	8.6	All out	10.1	8.5	8.5	10.9	10.3
883	8.6	8.2	....	9.3	7.7	8.0	10.4	9.8
972	8.6	8.3	....	9.2	7.6	8.0	10.4	10.0
1,105	8.6	8.2	....	9.2	7.4	8.0	10.3	9.5
1,190	8.2	8.0	....	9.0	7.5	8.0	10.4	9.6

TABLE II.

Lamp.	L.	M.	N.	O.	P.
Distribution factor.	1.10	1.00	.911	1.00	1.02
Hours from start.	C. P.	C. P.	C. P.	C. P.	C. P.
0	13.2	12.5	14.1	17.0	14.6
71	12.6	12.5	13.5	....	....
137	10.7	12 6	11.7	16.4	15.9
205	10.1	11.9	11.0	14.8	13.6
254	9.7	11.6	10.4	14.0	13.0
319	9.3	11.8	10.1	13.3	12.3
450	8.9	11.5	9.4	11.6	11.6
593	7.8	10.6	9.3	10.8	0.7
646	7.6	9.6	8.2	9.6	9.2
696	7.1	9.3	7.9	9.0	8.4
785	7.1	9.4	7.8	9.0	8.7
837	6.8	8.7	7.5	8.3	8.5
1003	7.1	8.8	7.4	8.0	8.4

Table III. gives the average current strength found for each make of lamp, at times given in the first column:

TABLE III.

Lamp.	Mean current strength in amperes.												
	A	B	C	D	E	F	G	K	L	M	N	O	P
Hours.													
0	.567	.532	.693	.639	.589	.532	.662	.613	.458	.547	.568	.609	.536
100	....	....	..	....	....	...	....	....	.444	.532	.557	.665	.534
107	.575	.545	.760	.668	.581	.519	.680	.629	....	....	....	..	...
275	.567	.540	.778	.655	.566	.516	.681	....	..	....	....	....	....
380	....	....	....	....	....	....	....	....	.425	.513	.528	.615	.511
555	.548	.540	.770	.633	.541	.504	.662	....	....	....	....	....	...
615		..	....	....	....	....	....	....	.418	.505	.519	.595	.495
790	.540	.535	.770	.619	.510	.498	.654	.581	...	....	....	....	
930	..	....	....	....	....	....	....	...	.417	.500	.508	.585	.476
1,105	.535	.529	.000	.611	.517	.495	.649	.577	....	....	....	...	....

Table IV. gives the mean percentage of original candle-power for each make at times given in the first column:

TABLE IV.

Lamp.	A	B	C	D	E	F	G	K	L	M	N	O	P	Average.
Hours.	%	%	%	%	%	%	%	%	%	%	%	%	%	%
0	100	100	100	100	100	100	100	100	100	100	100	100	100	100
100	93	94	106	101	92	93	95	93	87	100	87	95	107	95.6
200	86	89	109	99	84	86	90	87	78	98	79	87	97	90
300	79	83	106	93	77	81	86	83	72	94	72	80	86	84
400	74	80	102	82	72	77	81	78	67	90	67	72	78	78.4
500	69	73	98	75	66	70	77	74	63	86	63	66	71	73
600	64	70	94	70	61	66	73	70	59	80	60	60	65	68.6
700	61	67	91	66	57	62	70	67	55	75	56	55	60	64.7
800	58	64	90	63	53	59	66	63	54	73	55	51	58	62
900	55	63	All out	60	51	57	63	59	52	70	53	49	56	57.4
1,000	54	62	....	58	49	56	62	58	54	70	52	47	57	56.6
1,100	52	61	....	57	47	56	61	57	....	...	....	....	....	55.8
1,200	51	61	... .	56	47	56	61	57	....	...	...	....	....	55.4

Table V. gives the average watts per mean horizontal candle power for each make, at times given in the first column:

TABLE V.

	Watts per mean horizontal candle power.													
	A	B	C	D	E	F	G	K	L	M	N	O	P	Av.
Hours.														
0	3.0	4.4	5.4	4.3	4.1	4.1	4.2	3.9	3.8	4.8	4.4	3.9	4.0	4.2
100	4.3	4.8	5.6	4.5	4.4	4.3	4.6	4.3	4.2	4.7	4.9	4.5	3.8	4.5
200	4.6	5.1	5.6	4.5	4.7	4.6	4.9	4.6	4.6	4.7	5.3	4.9	4.1	4.8
300	4.9	5.4	5.7	4.8	5.0	4.9	5.0	4.8	4.9	4.8	5.7	5.2	4.5	5.0
400	5.2	5.6	5.9	5.3	5.4	5.1	5.3	5.0	5.2	4.9	6.1	5.4	4.9	5.3
500	5.5	6.1	6.2	5.8	5.7	5.5	5.5	5.2	5.5	5.1	6.3	5.8	5.2	5.6
600	5.8	6.4	5.4	6.1	6.1	5.8	5.7	5.4	5.8	5.5	6.7	6.3	5.[illegible]	5.9
700	6.1	6.7	6.6	6.4	6.5	6.1	6.0	5.6	6.1	5.8	7.0	6.9	6.1	6.3
800	6.4	6.9	6.7	6.7	7.0	6.4	6.3	5.9	6.3	5.9	7.2	7.4	6.2	6.6
900	6.7	7.1	....	7.0	7.2	6.6	6.6	6.2	6.4	6.1	7.4	7.7	6.3	6.8
1,000	6.8	7.1	....	7.2	7.4	6.8	6.7	6.4	6.2	6.1	7.5	7.9	6.2	6.8
1,100	7.0	7.2	....	7.3	7.6	6.7	6.8	0.5	....	..	....	....	....	7.0
1,200	7.1	7.2		7.4	7.6	6.7	6.7	6.5	....		....	....	.	7.0

From the data given in the tables plates I. to XIII. are constructed. Each plate refers to a single make of lamp. Plate I. gives curves relating to the average lamp of make *A*. In it curve *a* gives percentage of initial candle power, *b* gives watts per candle power, *c* gives candle power, *d* gives current strength, and *e* gives number of lamps burning. The time scale is common to all curves. The curves are lettered alike, are drawn to the same scale, and have the same starting points in each of the plates.

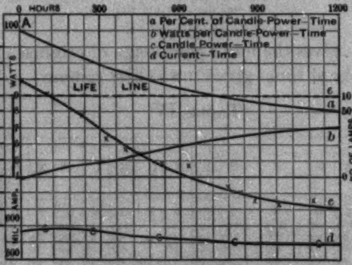

Plate I

The make of lamp referred to is indicated by the letter in the upper left hand corner of each plate. Curves *c*, *d* and *e* are plotted from observed values, but curves *a* and *b* are obtained from points 100 hours apart on *c* and *d*, combined, with the voltage 110 for *b*.

Want of time, due to the pressure of other duties, prevented our taking readings as often as we would like to have done, and prevented also the examination of a number of points of interest which developed as the test progressed. For the same reason we are unable to include the data and curves for the individual lamps.

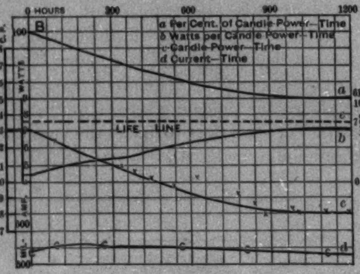

Plate II

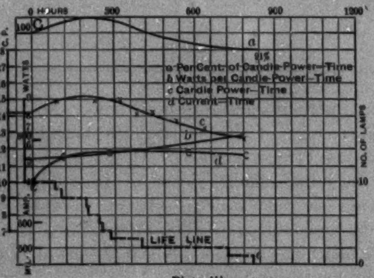

Plate III

Thirty-one lamps in all were broken, distributed as follows: All of the C's (10), 2 D's, 8 E's, 2 K's, 4 L's, 1 M and 2 N's. All the lamps of A, B, F, G and O makes survived, the O's for 1,013 hours, the others for 1,200. The average life of the C's was 386 hours.

Plate XIV. gives two sets of curves. One set gives the curve of general average of percentage of initial candle power with respect to time, derived from the averages of the several makes, and beside it, the curve of the make which showed highest percentage throughout, marked min. drop. make M, and also the curve showing lowest percentage, marked max. drop, make L. The other set shows the general average of watts per candle power, and also the curves of the two makes showing the maximum (make N) and minimum (make M).

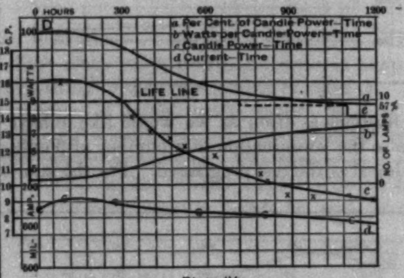

Plate IV

Examining the results first for candle power, we find the value 14.5 as the average of all the lamps tested at the time the current was first turned on them. Of the 13 separate lots, six only show an initial candle power of 16 candles (neglecting tenths), though one make which started at below 15 rises above 16 within the first 100 hours. The lowest initial candle power is found for make M. Makes G, K and O start at 17 candle, the highest figure reached at the start.

Looking at the curve which gives the average percentage of initial candle power, Plate XIV., we find that the lamps on the average grow dim at a steady rate, falling off to 90 per cent. in about 200 hours, to 68 per cent. in 600 hours, and end at 1,200 hours with a little less than 55 per cent. Deferring the consideration of the C lamps, the M lamps sustain their power best, losing only about six per cent. in the first 300 hours, and ending a little under 70 per cent. of their initial candle power. The L lamps drop rapidly on the start, losing 30 per cent. in the first 300 hours and ending at 54 per cent. The E's and the O's drop to 47 per cent. at the close, but sustain themselves better than the L's in the earlier part of the run.

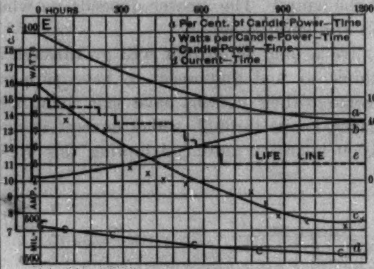

Plate V

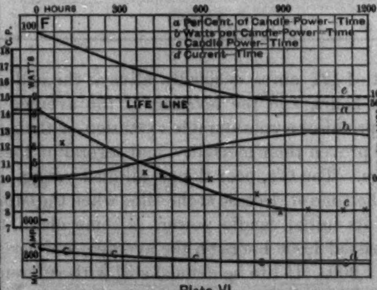

Plate VI

With respect to efficiency, using the term in its ordinary signification, as measured by watts per candle power (though in this sense the reciprocal is the proper measure), Plate XIV. shows an average initial value of 4.2, rising to 5.0 at 300 hours, 5.9 at 600 hours, 6.8 at 900 hours, and 7.0 at the end. The M lamps again have the best place, starting high but ending low, at six watts per candle power.

But what shall we say of the results as bearing on the question of the life of the lamp? The answer depends on what we shall define the life of a lamp to be.

Is a lamp alive as long as current will go through it? Some advertisers think so, and they seem to have support in their opinion, for some of their patrons have sent them, with complimentary remarks, lamps through which current is said to have passed for 6,000 hours or more. Adopting this view, our test shows 96 out of 127 lamps decidedly alive at the end of an average of 1,120 hours, and the leveling of the average curves of Plate XIV. promises a considerable extension of their existence; 31 only have proven their mortality, and several of them lived to a good old age. The C's, poor things, all died young.

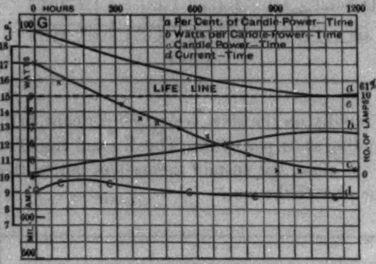

Plate VII

If we set the time of decease at the time when the lamp gives 50 per cent. of its initial candle power, as some claim, the lamps tested have passed 1,000 hours on the average, and certain makes have passed 1,200. If life be defined still with reference to percentage of initial candle power, but the limit be placed at 75 per cent., some individuals are still alive, but on the average, all died at 450 hours; 80 per cent. as limit cuts them off at 330 hours, and 90 per cent. at 180.

But there is yet another way of defining the life of a lamp. The one who ultimately pays for the lamp is the one whose premises are lighted by it, and the thing he is supposed to pay for is light. For him, the lamp is dead when it ceases to give a certain candle power, without reference to what its candle power was when it was new. His premises, let us suppose, are properly lighted by a certain number of new lamps which actually give 16 c. p. But when their light drops to 14 (12½ per cent.), he is no longer satisfied (and he ought not to be). For him the average lamp lasts about 240 hours. But if he is at liberty

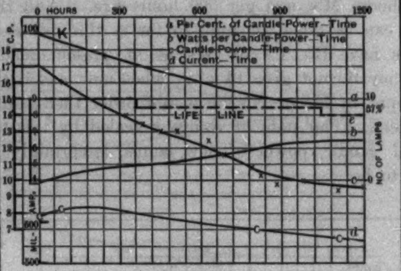

Plate VIII

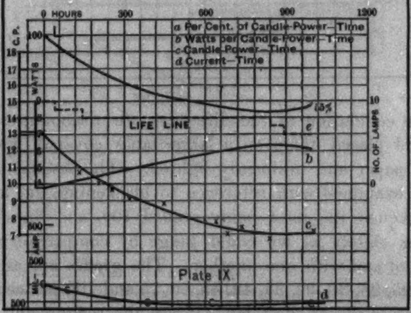

Plate IX

to choose the make of the lamp, without reference to its efficiency, he will probably call for lamps of the G make, and get 400 hours service from them. If, however, he can induce the maker of the M lamp to make a lamp giving 16 C. P. when new, and having the same characteristics as those we tested, he will choose M's, and get 500 hours life. And that is the best that can be done for him, so far as this test shows. He would not have B's, L's or M's, as at present made at all, because they do not give 14 C. P. when new, and if the station manager put in F's or N's he would order them out in less than a week. The state of things revealed by this test, as to the actual candle power of new lamps, is certainly not what it ought to be.

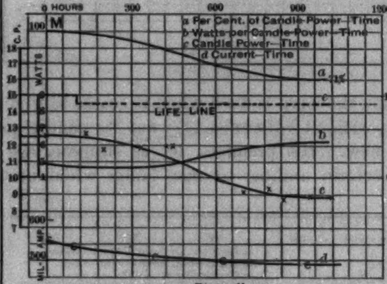

Plate X

At the conclusion of the life test we dismounted all the lamps and placed them on a large table covered with clean drawing paper and uniformly lighted. The several lots were carefully examined with reference to blackening, the lot which was blackened the most being graded 100, and the other lots graded as closely as we could judge by comparison with the blackest lot and with one another. The grades are as follows: 10, 12½, 25, 28, 30, 32, 35, 42, 45, 50, 75 and 100 per cent. of the blackening of the blackest lot. When the sorting and grading was satisfactorily completed, and the letters on the lamp bases examined, it was remarked that *the two lots which were least blackened were of the makes which are advertised as exhausted* *by mechanical pumps.* That they should both be so much alike in blackening, and both only one-half as much blackened as the least blackened of the mercurially exhausted lamps, is, to say the least, worthy of notice. It is, of course, not established that the observed freedom from blackening is due to the absence of mercury vapor in the lamps, as the makers claim, but if investigation should establish the claim the mercury pump will probably become once more a laboratory instrument. [The lot of lamps graded 10 per cent., and the lot graded 100 per cent., were

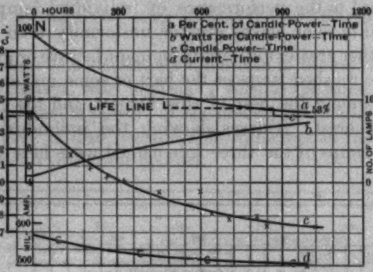

Plate XI

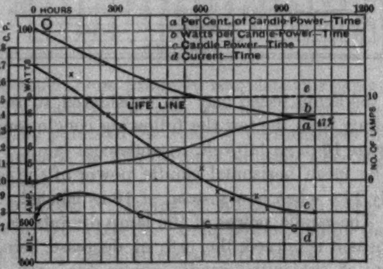

Plate XII

mounted together on cardboard and exhibited.] As the giving of the letters designating the several lots of lamps, with the percentages of blackening, would, after the above statements, serve to identify the makers in the preceding tables and curves, the letters are omitted.

With regard to the performance of individual lamps of each lot, we found in some makes greater uniformity than in others, indicating either greater uniformity in manufacture or greater care in sorting the finished lamps on the part of some makers. The A lamps were among the best in this respect, and the C's and F's among the poorest. [Curves showing the individual performances of the lamps of the three lots named were here shown and discussed.]

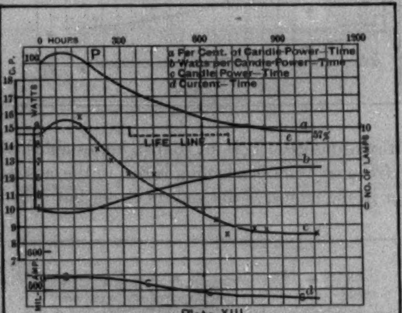

Plate XIII

So far as the lamps tested may be considered to fairly represent it, we draw from the test the following conclusions concerning the average 16 c. p., 110 volt, incandescent lamp:

First. The mean horizontal candle power of the lamp as defined, is only 14.5 when new instead of 16 as labeled.

Second. The candle power of the lamp diminishes at a fairly uniform rate as it is burned, the rate of decrease being roughly 10 per cent. of the initial candle power for each 200 hours burned.

Third. The lamp makers' claim, that the lamp will burn for 600 hours before breaking, is more than established, 96 out of 127 lamps being unbroken at the end of over 1,100 hours.

Fourth. Defining the life of a lamp to be the number of hours it will burn before it will fall from 16 C. P. to 14, we find the life of lamps in all respects like those tested, but so made as to give an initial candle power of 16, is 240 hours.

Fifth. The initial efficiency of the lamp is 4.2 watts per candle power. At 600 hours it is 5.9 watts and 1,200 hours 7 watts. The average efficiency during the first 600 hours is 5.04 watts.

Sixth. There is considerable difference in the rating of lamps by different makers, who nominally furnish the same article, the average initial candle powers of the several lots ranging from 12.5 to 17.0. There is also considerable difference in the unformity of the product of different makers.

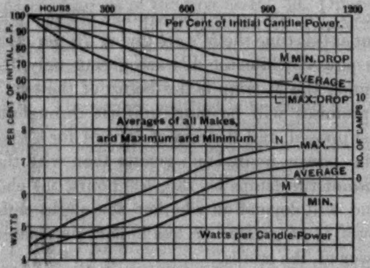

Plate XIV

The above conclusions properly complete this paper, so far as the purpose for which the test was undertaken is concerned. Some interesting points in the relative performances of different lamps may perhaps be alluded to in addition, since they cannot affect the makers.

Referring to Table V., lamps A, K, L and O have the same initial efficiency (3.9 watts per candle power), and for the first 700 hours A and L keep together, rising to 6.1 watts. K does better, rising only to 5.6 watts per candle power, while O rises to 6.9 watts. Referring to Table IV., there is greater difference among the makes named, the K's at 700 hours giving 67 per cent. of their initial candle power, while the A's give 61 per cent. and the L's and O's only 55 per cent. Of the four makes therefore the K's are best, since they maintain their candle power better and have the highest average efficiency. The differences, however, are so small as to make noteworthy the similarity between lamps of the same class, coming from different factories, as in this case. Comparing the F's with the G's in the same table, the F's have a slight advantage in efficiency on the start, but they come together in 500 hours. The G's, however, have such an advantage in the way they hold up in brilliancy as to balance the slight difference in efficiency and make them the better lamp.

The most interesting result, however, arises from a comparison of the lamps L and M. The L's have the highest initial efficiency (3.8), and the M's the lowest (4.8), excluding, of course, the C's, which can not be classed with good lamps, but after 250 hours burning the efficiencies are the same (4.75). At the start the L's are furnishing light at the least cost, but after 250 hours the M's have the advantage in this respect. If we suppose both makes to be used for 700 hours, the average efficiency is the same for both. But at this time the M's are giving 75 per cent. of their initial candle power, while the L's are giving only 55 per cent. For the consumer the M lamp is the better of the two, since the lighting of premises will be more uniform throughout the 700 hours of use supposed, and the cost of current for a given aggregate of candle power hours is no greater than with the L's. If the time of use of the lamps be, say, 400 hours, the choice is not so evident, and will depend on whether the slight difference in cost (watt hours) is of greater or less importance than constancy of candle power of the lamps.

A rough graphic comparison between the several makes of lamps may be made by considering the areas between the curves *c* and *a* of the several plates. The curve *a* is evidently parallel (raised three divisions) to the curve *c*, which would have been found if the initial candle power of each lamp had been 16 as labeled, other things being unchanged. The more nearly horizontal *a* and *b* are, and the lower *b* is, the better the lamp. As drawn, the curves can only give qualitative results by such comparison, but if the scale to which the *b* curves are drawn were properly chosen, the area comparison might give more accurate conclusions as to the relative merit of the several makes. It is clear that the form of the area between the *a* and *b* curves in Plate X. is nearer the ideal than any other, and that Plate XI. is poor. We are inclined to the belief that facts such as those alluded to in the comparison of the lamps L and M may lead to a modification of the views commonly expressed as to high and low efficiency lamps.

DISCUSSION.[1]

THE CHAIRMAN [Vice-President Hering.] We can be congratulated on having such a paper as this one, at our meetings. It is papers of this kind that are read with great interest by electrical engineers all over the country and bring the Institute such a good reputation. Any one who has ever attempted tests of this sort will know how very tedious it is to carry on such a long set of observations. There are a number of gentlemen in the room who have worked in this same field from whom we would be very glad to hear.

PROFESSOR ROBERTS:—With reference to the comparative testing of lamps—there are three methods:

1st. Running lamps at the voltage marked and measuring the C. P. and current during life, and plotting the curves in the manner Prof. Thomas has done. 2d. Running all lamps at the voltage at which they originally gave the C. P. marked, and in other respects proceeding as in the first case. 3d. Operating all lamps at the same efficiency. It is impossible to make comparisons when the test follows the first plan. Although the paper is of interest and valuable, it does not, to any extent, give comparative facts. The same objections hold good, for the most part, to the second plan. The proper method is to obtain lamps of the same efficiency; and if to be of the greatest value, as near as may be of the efficiency desired to use; and start them on the same basis. All large manufacturers of lamps make any efficiency desired; we have orders for 16 C. P. lamps between the limits of 50 watts and 75 watts—our standard being 55 watts.

Prof. Thomas states lamps purchased in the open market vary in C. P. I desire to mention the lack of knowledge of what is wanted on the part of the customer. In all probability the lamp will not be used within 4 per cent. of the voltage ordered for the greater portion, probably all of its life, and such a departure in the matter of voltage will make more difference in C. P. than is likely to exist in lamps sent out by the better manufacturers, although not by all. Probably 50 volt lamps are going to burn at 54 volts and 110's at 114 to 116 volts.

With reference to the life of the lamp and to blackening. It is true of some stations that when the lamp becomes blackened it is considered dead and removed, but in many stations the following method is pursued:—The manager receives a complaint that some good customer has not light enough, and he instructs his superintendent to "raise the voltage a little." The next day he visits the customer and all are happy. When the end of the month comes and the "lamp renewals" report is shown him he immediately complains to the lamp manufacturer. I recently had occasion to inspect a large isolated plant. The office had the lowest voltage (the distribution was very bad, owing to additions of lights since original installation and without increase of wires), the consequence was that the lamps had lasted for thousands of hours, but were very black and were giving about four C. P. In the warerooms, where plows and other bulky articles are stored, the lamps gave about 25 C. P., and had a short life. I advised the renewing of all lamps in the office the first of each month (statement time) and taking the old lamps for the warehouse.

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[1] This paper, as originally presented, was discussed at length by Mr. J. W. Howell, and has since been revised by the author. In view of the changes made therein, Mr. Howell's remarks, as well as those of the author in reply have been withdrawn.—EDITOR.

The paper does not touch upon the fact, that the source of most of the complaints about lamps is the lack of appreciation of the effect of variation of voltage on life. I have been a manager and superintendent of electric light stations as well as a manufacturer of lamps and, therefore, have had experience on both sides. There are many places which we have investigated where practically, the above effect was not appreciated in the least. Many do not know what voltage they actually have at the lamp even at one time, and "suppose all the lamps are at the same voltage at all times as they take care it should be so *at the station.*" As it is impossible to have actually constant voltage, and as one can not tell a customer that he does not know what he is talking about, it is only by saying, "of course, as you know," and "you know that," etc., that, in the course of time, he does know it. There is no "manufacturers standpoint." There is no reputable manufacturer but desires his customer to know all possible about the goods sold. The more the customer knows the better for the manufacturer. It is by personal missionary work and by the publication of such papers as the one we have just heard that the conditions necessary for the most satisfactory use of the lamp can be made known to the average purchaser of incandescent lamps.

A paper read at the General Meeting of the American Institute of Electrical Engineers, Chicago, Ill., June 7th, 1892, Vice-President Hering in the Chair.

SOME POINTS FOR THE ELECTRICAL ENGINEER.

BY HORATIO A. FOSTER.

Although it is the province of this Institute to consider largely the more theoretical side of electrical engineering, and rightly so, it is perhaps well that it should occasionally have brought to its attention that outside practical part, only for which theory would have no backing and therefore could not reach its proper development.

It has been the fortune of the writer during the last ten months to visit and inspect, both technically and financially, considerably more than a hundred central lighting stations.

Taken as a whole, it really seems as if much of the ordinary commercial common sense and the usual keen judgment of self-interest had been on a vacation when these plants were erected. In many cases this traces back—first, to the wiles of that slickest of all individuals, the agent, who is always willing to assure the buyers that his system of lighting will virtually run itself, and then to the fact that people were formerly so doubtful of the prospective profits that on going into the business they invested the smallest amount that would start the work. The consequence was, bad construction, bad machinery, bad lines, and, of course, an entirely unreliable service. Oftentimes the man put in charge was a novice, and that worst of all novices for the business, the local amateur electrician; and is it any wonder that the business was a failure for the first few years?

In some localities this condition still obtains, nothing having been done but to patch up the bad work of the past.

In a few cases the administration has been changed by putting at the head some substantial and tried business man, very much to the benefit of the concern. While, in many instances, even the large city plants suffer from the above causes, they also suffer from other conditions inherent in the size of their field.

The pioneers were so timid about the outcome, that the city station was invariably very much too small, and inside of two years had become so patched up and crowded, that rebuilding became necessary, and this in many cases was repeated two or three times.

There is probably no business extant which has been built over so many times and that has had the same quality and quantity of brains applied to it that is in such chaos as to the proper design of building and arrangement of machinery. In cotton or any other weaving industry, machine shops, foundries and similar establishments, it has long been determined which is the best form of building to erect, and those put up in the last ten years or so of a similar industry are planned in the same lines, differing only in size to suit the special case. Now, this not only fails to be the case in electric lighting, but outside of the two prominent systems of connecting the dynamo to its power—that is by direct belting to fast running engines, or by counter-shaft to larger slow running engines, there are scarcely a half-dozen stations in the state built on the same or similar lines.

So much diversity of opinion is shown in designing these stations, and oftentimes by those whose opinion should have no weight, but unfortunately does, that it has the effect to convince some of the investing public that the electrical engineer does not know his business and they might as well go it alone.

If some action could be taken by this Institute through a committee which should examine into proper station design and report on some few general plans and suggestions as to lines to be followed, it might go far toward establishing a better backing for our members who are in that branch. As it is I think it will be acknowledged, that far more electrical engineering skill is applied to the larger isolated plants; and that the central stations are enlarged or rebuilt under the management of the local superintendent or board of directors, who frequently lack technical hnowledge or even familiarity with recent improvements elsewhere. Quite often this is owing to the fact that the only experience they have had with an electrician has been the generally young inexperienced expert who was sent out to install the first plant, and they think they can make better use of their money by controlling it themselves.

The method of distribution should also have consideration, especially in reference to the amount and style of underground work which certain sizes of city will be able to stand.

This may seem a large question, but you can be assured it is one of the most vital to the future welfare of the industry. It may be said that it is not well to interfere in private business, and that electrical engineers can be trusted to remedy the evil. I assure you the electrical engineer individually "is not in it," to use a vulgar phrase, and sad to say, has never been in it to any extent. Quite often officials of the electric lighting companies take a jaunt through the country visiting different stations for models for a new one for themselves. This often results in a worse muddle than ever, as they seldom find two stations at all similar, and they return resolved to use their own judgment.

This leaves the real electrical engineer out in the cold, at a point where if known he could by consultation do much good but many of these company officials never heard of the American Institute of Electrical Engineers and need to have it brought to their notice in some way.

Some may think that in time the electrical engineer will be called in and consulted, as are engineers of other works, but at the present rate I am afraid many will be angels or worse before the majority of electrical central station companies come to that point.

Perhaps if instances be mentioned under the following heads a better understanding can be arrived at, viz.: Location; foundations; construction of station; motive power; arrangement of machinery, and location and construction of lines.

LOCATION.

The location of plants is not now so generally bad as in the past, although we occasionally find a station in a location for which no excuse is apparent, as, for instance, one located in the basement of a carpenter shop at a long distance from fuel supplies, and only plenty of condensing water to recommend it; insurance rate, three per cent. or more. Another located nine miles away from a village of two thousand people to take advantage of water power never surely available all the year, and since destroyed entirely for the greater part of the year. Another placed so low down in reference to its water power as to be flooded in freshets. Another located more than a mile from fuel supplies, and the center of distribution at the top of a long, steep hill, with not even free water to recommend it. Another so located in relation to a very intermittent water power as to be unable to take advantage of the water for condensing purposes and now running the poorest kind of a slide-valve engine. Another located in a rickety, old, uncared-for part of a gas-house, where coke is scarce, and apparently plenty of water power available within a half mile.

Another located a long distance from center of distribution and coal supplies, to take advantage of cheap land and cheap fuel, the last of which has since proved very unreliable, when seemingly land could have been had much more central, with a much surer supply of cheap fuel, and to cap all, the value of that land has since increased enough to have paid the entire cost of the station at any moment it be desirable to sell.

One advantage of some of the older centrally located stations has since been learned in the increase in value of such real estate. One such station visited will enlarge and rebuild soon, and will sell its present real estate for enough advance to go far toward paying for the new station, and there are others which can, I think, tell the same story. In fact, in the present problematical growth of the future, a well-selected piece of ground may prove a very valuable part of the investment.

FOUNDATIONS.

Foundations are now much better than in the past, and some of those now placed under dynamos would sustain almost anything that could be put on them. Poor foundations are occasionally seen though, as for instance those under a large bank of boilers in a station which was erected and started in six weeks. The boilers have since lurched forward until the fronts are some two or three inches out of plumb. In fact too little attention seems to have been paid to boiler foundations as a general thing.

Engines have fared better and when the ground has been good the foundations are to be little criticised.

One station has very large Corliss engines and owing to water soaked ground and limited time in which to build the brick foundations, they were placed wholly above the floor level of the dynamo room, and belts run both ways from the fly-wheels to shafting on either side of the room.

Very few bad dynamo foundations are now met, but many unbalanced armatures are found which no amount of foundation would hold steady.

STATION CONSTRUCTION.

In station construction there has been no settled design, therefore the promoters have generally built as they saw fit, occasionally following suggestions of the agent. Of course many plants are located in buildings already up, in which case the machinery had to be made to fit.

In many of those built especially for the work, no attention has been paid to designing for a low insurance rate, and brick stations are found sheathed all over the inside with pine, the one thing that insurance inspectors always criticise. A couple of coatings of whitewash or paint would have been much cheaper, safer, and looked quite as well.

When stations have been remodeled, very little improvement has been made in the smaller ones, but those in large cities have generally taken advantage of all the improvements to date, if not too strongly attached to some one parent company.

MOTIVE POWER.

The selection of motive power has of course been governed by local conditions; in the northern and northwestern part of New York state, water power is abundant and is largely used as motive power for stations, but almost invariably with steam reserve.

Water power alone has been found very difficult to govern for uniform speed, and some of the devices used for that purpose are to say the least ingenious.

When the load is large enough to use part steam there is no trouble whatever, as the wheel takes a steady load and the engine does the governing.

Plants are sometimes placed a long way from the work in order to take advantage of water power and in some cases it seems as if all else had been sacrificed to that one point.

Of engines there is no end to the different design, style and size used. Some use high speed, belted direct to dynamos, others slow speed belted to counter, some simple non-condensing where compound condensing would be decidedly best.

I know of at least three plants located in water works pumping stations, using high speed simple non-condensing engines on a practically steady load, all of which have been erected during the past three or four years.

Two stations I remember having an all night and every night load of arc lights and situated on a lake shore which run high speed simple non-condensing engines.

To offset this there is one station that deserves mention as it embodies many points of value. The building is substantial, of brick, two stories high and on a river bank. The boilers are very efficient and well set, the engine is a moderate speed compound condensing, with shaft extended for three dynamo pulleys, and one for the power condenser. There is a heater between the exhaust and condenser and a power pump attached to one side of the condenser. The flow to the boilers is governed by a bye pass valve, as the pump is of course in motion all the time the engine is running. This station has only an arc load and runs all night; it was built entirely under the supervision of a local mechanical engineer with the exception of connecting the lines and dynamo terminals to the switch-board, and would, I think, be hard to beat for results.

ARRANGEMENT OF MACHINERY.

The arrangement of machinery is as diverse as the style of motive power; when direct belting is used the arrangement is generally simple and plain, but oftentimes so crowded as to be very inconvenient.

There seems no end to the arrangement of shafting, some have a line shaft through the middle of the dynamo room and belt both ways to dynamos, others a shaft on either side of the room belting to dynamos in the center, others the shaft under the floor belting up to dynamos and occasionally to engines also, others have the shafting overhead, in fact, there is no end to the styles, some good and some bad. In most cases friction clutches are used with shafting for stopping individual dynamos and some few may use loose pulleys on independent sleeve for the same purpose.

The length of belt centers varies greatly, some apparently thinking if long belts are better than short, very long ones must be much the best.

The improvement in switch-boards seems to me more marked than in any other one department, which is probably owing to the pressure of insurance men who look at the switch-board as the most dangerous fire risk of all.

LINE CONSTRUCTION.

In line construction I cannot see that any very marked improvement has been made, excepting in some cases, the substitution of larger and better poles for the older ones.

The use of black covered wire in place of white has perhaps made the lines a trifle less conspicuous but that is about all. The lines still have the apparent aimlessness of direction mentioned by Prof. Thomson at Buffalo.

People in the larger interior cities are already agitating the question of underground wires and it seems probable that unless something is done soon by the companies themselves to rid the streets of the more conspicuous of these wires, there will be very costly trouble for them to contend with.

It can no longer be denied that wires are working well every day in sub-ways and the only valid argument against them is extreme cost. Nevertheless when the general public learns this fact, cost will have little to do with the argument.

In many places wires can be removed from the main streets to alleys back of the shops, and the extra cost will be decidedly cheaper than underground work. In such few places as this has already been done, wires are inconspicuous and the underground question is never raised.

In street lighting the favorite method is the cable suspension across the street. Where the work is neatly done this serves very well but it would be difficult to find a more unsightly thing than some of the lamps so suspended.

Considerable trouble is occasioned in the above method by breakage of the wires at the joint with the line, and at the cross bar over the hood. There is no apparent reason why twin or duplex wire should not be more used for this purpose, instead of the kinked and straggly single wires.

There are many other methods, some of them good in principle but bad in the application, and so it goes.

It seems to me that if some action was taken by a non-partisan association like this Institute, an advance toward better engineering might be made, if a few general laws could be suggested to be followed in laying out the different parts of a lighting station. Perhaps it may be said that much of the above work should more properly come before the National Electric Light Association, but as many of that society are members of this Institute, I feel sure they would individually appreciate its backing in the future when the time comes for enlargement, and they have need of combined expert opinion.

DISCUSSION.

MR. H. WARD LEONARD:—Mr. Chairman, I think the points that Mr. Foster has made in his paper are very good ones, and that they will be appreciated by all who have contact with central stations that have been constructed in the past. I know that in my own case I have quite recently had it forcibly impressed upon me in the case of two central stations of considerable size. In one case I remember that a very large central station was to be erected, not a thousand miles distant from here, and considerable time was spent in the selection of a proper site. At that time I remember personally taking sufficient interest in the matter to make calculations as to what would apparently be the best site for a central station to meet the proper demand. The question of the value of the real estate was a matter which proved to be quite important in determining upon the selection, and the first selection had to be abandoned very shortly and a second selection was made, and a very excellent and a very profitable central station was established. But it is a little gratifying to learn, as I have recently, that the central station is about to be moved to within one block of the site that seemed to me personally to be the best, and it could have been placed there originally at a very large saving over what it can be now.

The second instance that has come to my attention recently is one in which a central station has expended something over a quarter of a million of dollars, has made two changes of location, and finally the parties owning the station called me in with the view of learning as to whether certain changes and improvements could not be effected, and it was almost sad to notice that so much money had been expended, and that they were still so very far from economical results. The station was one which had ample space to place apparatus sufficient in capacity to run all of the electrical service for a town of three or four times the size of the one in question. They had abundance of space originally, but they had failed to utilize it advantageously. The building was full of the very long belts which Mr. Foster has very forcibly called attention to, occupying a tremendous amount of space, a large number of friction clutches were used, long steam pipes, and very poor arrangement of all kinds of apparatus—in fact, so perfect a disregard had been evidenced of the proper condensation and arrangement of machinery that although they had but slight capacity they found themselves in such a position that they could scarcely enlarge their facilities without entirely remodeling the station. That was relative to the station itself.

Outside of the station I found that a circuit had been established originally for a very contracted area, and that the system of conductors had been introduced at moderate loss and with a very slight cost in copper, no survey having been made originally that would enable them to judge fairly well as to the location of the probable load. It appears that they got loads at entirely unexpected places, and consequently they got very bad distribution of current relative to the copper which they have installed, with a consequent very bad condition of pressure throughout the system. At this time the question might have been submitted with very beneficial results to some competent engineer. But as soon as they realized that it meant either a readjustment of matters or more copper they came to the conclusion that there was no difficulty about putting in more copper because the territory was limited. and if they got in more copper they realized they would equalize the pressure. Consequently a large number of loops were placed all around their system which made the loss of the system one-third or one-half, perhaps, what it originally had been. In fact, so perfect a disregard had been shown of the question of feeders in that system that they ran out from the station a distance of about 300 feet, three wires of 0000 guage that they didn't have use for, and connected directly on to the mains. Probably the maximum loss of that line did not exceed two or three per cent. Now they want to enlarge. Of course enlarging a direct current plant, as this was, and extending a considerable distance on a basis of two or three per cent. loss in feeders, made the cost of copper prohibitory, and they are now facing the problem of remodeling the entire system in order to put in sufficient loss in those feeders to enable them to operate at reasonable distances with fair economy.

I think that there is one point that Mr. Foster has already touched on which is one of the most conspicuous fallacies in this business. He called attention to the fact that the ordinary purchaser realizes that long belts are better than short belts, and comes to the conclusion that a longer belt is better yet. That seems to be equally true as regards conductors and engines and a good many points. The ordinary purchaser has an impression that you cannot make the engine too large and you cannot make the conductors too large; that the bigger they are the better. But they find that such a plant is operated at very poor economy. They find themselves operating under conditions which make it impossible to enlarge, and altogether find themselves very much restricted as regards their extensions in the future and the profits from their system because of the impression which is so prevalent that it is impossible to make the apparatus you use too large.

There is one thing which I think is essential for any first-class station in starting out which seems not to be considered except by a few, and that is this:—Any gentlemen who are contemplating the investment of money in a central station should have a great deal of money spent under the supervision of a thoroughly competent and experienced person to determine exactly where such a station should be placed, exactly what extent of demand there will be, judging by the existing establishments to be supplied, how large the plant should be to start with, what the arrangements should be for extension, and all such matters as that. But in practice we find that where gentlemen are going to build a station they have a lot that they have selected that is a little ways out of town and they have rented a building, or have made arrangements to do so, perhaps put it in an old factory or an old foundry or something of that kind, and they want a plant of about a thousand lights to start with. The town is about two miles square, and they don't know where the lights are going to be, and they only want to provide in the beginning for conductors, lamps, and so forth, for about two hundred lamps, but they would like dynamos and engines for about a thousand lights, and ask for figures. That is the way they figure at the start. They show a total disregard for the location of the load with reference to the station. They do not care anything about the distance, whereas they would have got far better results in economy had they located centrally.

PROF. ROBERTS:—Because I am not now in construction work I desire to speak in this matter of obtaining proper engineering and paying for it. Architects, as you know, charge 3½ per cent. for plans and 5 per cent. for plans and supervision. How few companies pay such a percentage to a consulting electrical engineer.

With reference to the erratic arrangement of buildings, it is not very long since I had occasion to look at a plant that had been under three different managers, and each manager had put in a boiler room and the boiler rooms were in different places. They started in with an engine room, dynamo room and a boiler room. They had it on the corner of the lot. The next manager wanted more boiler room and put a boiler over here, and the next man put a boiler room down here (indicating) and they had three boiler rooms, three engine rooms and three dynamo rooms; they had no scheme drawn up originally as to what they would do when they extended.

MR. T. C. MARTIN:—Mr. President, I think it is due to Mr. Foster to state that one reason for the somewhat general terms in which he has couched many of the statements in his paper is the fact that he is at present an officer of the government. He has been investigating these things as an agent of the Census Office, and he is not at liberty to go into details as he will at some later time. The paper is certainly as remarkable for that which it does not state as for that which it does state. One of the remarkable features of this paper is that after visiting a hundred central stations, of which I believe nearly the whole were in New York State, there is not the slightest mention made of the use of the storage battery in a single one of them. It seems to me that in that respect at least we must be very much behind our compeers in Europe. As nearly as I can ascertain, at least 10 per cent., if not more, of the central stations in Europe are equipped with storage plants, some of which have been running four or five years. There are not a great many central stations in Europe, but still the fact that so large a percentage of them should have found it profitable to employ storage batteries must convey some suggestion to us. There are some gentlemen on the floor who have had sad experiences in the days gone by with station storage batteries, but it seems to me that we must have advanced a little bit since the days of our pioneer work, and that the time has arrived when we might learn something from what is being done in Europe.

A point Mr. Foster alluded to was painting in stations, and I believe it was once stated by a distinguished expert that dynamos might be painted any color without increasing their efficiency. I believe that the color that the central station is painted is apt to make a great difference in its efficiency. I have heard of one station, I think it is in this state, at Jacksonville or Dixon, where the interior is as sumptuous—well, at least as that of the office of the average electrical engineer. The whole of the interior, is a dazzling, glittering white; and even the engines have been touched up in a style that equals in gorgeousness the splendor of Barnum's circus wagons. Now I understand that that station is paying as much as any station in the state. I have been told, in fact, that its dividends have exceeded those of any other station; and it seems to me that there may be a connection between the cleanliness of that station and the dividends which it pays.

On motion duly seconded and carried, the President appointed as a committee on acknowledgment of courtesies extended to the Institute, Dr. Charles E. Emery, Prof. E. P. Roberts and Dr. E. L. Nichols.

[Recess.]

Note.—The proceedings of the General Meeting will be continued in the next issue.

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